This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

Physical review

American Institute of Physics,
American Physical Society, Cornell University

5
D

2569
+2160

VOLUME X NUMBER 1

THE
PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, and FREDERICK BEDELL

L

JANUARY 1900

TABLE OF CONTENTS

A Determination of the Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether. I — **R. A. FESSENDEN** 1
Polarization and Internal Resistance of the Copper Voltameter — **B. E. MOORE** 34
A New Method of Comparing Two Self-Inductances — **H. V. CARPENTER** 52
New Books: *Wüllner*: Lehrbuch der Experimental Physik; *Ostwald's* Klassiker der Exakten Wissenschaften, Nos. 99 and 100; *Lengfeld*: Inorganic Chemical Preparations; *Hillyer*: Laboratory Manual 63

COPYRIGHT, 1899, BY THE MACMILLAN COMPANY

PUBLISHED FOR CORNELL UNIVERSITY
BY THE MACMILLAN COMPANY
LANCASTER, PA., AND NEW YORK
LONDON: MACMILLAN AND CO., LTD
BERLIN: MAYER AND MUELLER

THE PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, AND FREDERICK BEDELL

Two volumes of THE PHYSICAL REVIEW are published annually, these volumes beginning in July and January, respectively, and containing at least five numbers each. The price of subscription is two dollars and fifty cents a volume (five dollars a year), or fifty cents a number. Subscriptions should be sent to the publishers. THE MACMILLAN COMPANY, 41 *North Queen Street*, *Lancaster*, *Pa.*, or 66 *Fifth Avenue*, *New York;* Messrs. MACMILLAN & CO., LTD., *London;* or to Messrs. MAYER AND MUELLER, *Berlin.*

Previous to Volume V. (July–December, 1897) THE PHYSICAL REVIEW was published in annual volumes, each containing six bimonthly numbers, beginning with the July–August number, 1893. These may be obtained from the publishers at the former subscription price, three dollars per volume.

Subscribers who desire to have their copies of the REVIEW sent to them with the edges cut, can so obtain them by informing the publishers to that effect.

Correspondence relating to contributions should be addressed to the editors, at *Ithaca*, *New York.*

Manuscript intended for publication in THE PHYSICAL REVIEW must be communicated by the author; when publication in other journals is contemplated, notice to that effect should be given.

The authors of original articles published in the REVIEW will receive one hundred separate copies in covers, for which no charge will be made; additional copies, when ordered in advance may be obtained at cost.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

THE

PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND
THEORETICAL PHYSICS

CONDUCTED BY
EDWARD L. NICHOLS, ERNEST MERRITT,
AND FREDERICK BEDELL

VOL. X

PUBLISHED FOR CORNELL UNIVERSITY
THE MACMILLAN COMPANY
NEW YORK LONDON
BERLIN : MAYER AND MUELLER
1900

LIBRARY OF THE
LELAND STANFORD JR. UNIVERSITY.
a. 46887

COPYRIGHT, 1900
BY THE MACMILLAN COMPANY

NOV 19 1900

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

CONTENTS OF VOLUME X

L. JANUARY, 1900

PAGE

A Determination of the Nature of the Electric and Magnetic Qualities and of the Density and Elasticity of the Ether. I. R. A. FESSENDEN. 1

Polarization and Internal Resistance of the Copper Voltameter. B. E. MOORE 34

A New Method of Comparing Two Self-Inductances. H. V. CARPENTER . . 52

New Books: *Wüllner:* Lehrbuch der Experimental Physik; *Ostwald's* Klassiker der Exakten Wissenschaften, Nos. 99 and 100; *Lengfeld:* Inorganic Chemical Preparations; *Hillyer:* Laboratory Manual 63

LI. FEBRUARY, 1900

The Influence of Electrification on the Surface Tension of Water and Mercury. ERNEST MERRITT and SAMUEL J. BARNETT 65

An Objective Representation of the Hysteresis of Iron and Steel. KNUT ÅNGSTRÖM 74

A Determination of the Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether. II. REGINALD A. FESSENDEN. . 83

Thermodynamic Notes (No. 10); Revision of Some Thermal Data Concerning Benzene. K. TSURUTA 116

An Efficient Boyle's-Law Apparatus. W. J. HUMPHREYS 123

New Books: *Scheiner:* Strahlung und Temperatur der Sonne; *Gerland und Traumüller:* Geschichte der Physikalischen Experimentierkunst 126

LII. MARCH, 1900

On the Metabolism of Matter in the Living Body. EDWARD B. ROSA 129

A Dissociation Theory of the Electric Arc. C. D. CHILD 151

The Effect of Magnetization on the Elasticity of Rods. II. JAMES S. STEVENS 161

The Influence of Temperature, Pressure, Used Solutions, and Size of Anodes on the Deposit of Silver Voltameters. J. F. MERRILL 167

A Lecture Demonstration in Wave Motion. WILL. C. BAKER 175

An Apparatus for the Determination of the Coefficient of Expansion of Air at Constant Pressure. C. F. ADAMS 178

On the Change in Length of Soft Iron in an Alternating Magnetic Field. LOUIS W. AUSTIN 180

New Books: *Wentworth and Hill:* Text-book of Physics; *Franklin and Williamson:* Alternating Currents; *Aimé:* De l'influence de la pression sur les actions Chimiques; *Duhem:* Mechanique Chimique; *Miller:* American Telephone Practice 187

LIII. APRIL, 1900

The Acoustic Analysis of the Vowels from the Phonographic Record. LOUIS BEVIER, JR. 193
Note on the Variation of the Specific Heat of Water Between 0° and 100° C. H. L. CALLENDAR and H. T. BARNES 202
The Magnetic Molecular Susceptibility of Salts of the Iron Group. O. LIEBKNECHT and A. P. WILLS 215
Gibbs' Thermodynamic Model. W. P. BOYNTON 228
On the Temperature of the Acetylene Flame. EDWARD L. NICHOLS . . 234
Note on the Law of Distribution of Velocities Among Gas Molecules. N. D. C. HODGES 253
New Books: *Crew:* Elements of Physics; *Watson:* Text-Book of Physics . . 254

LIV. MAY–JUNE, 1900

The Transmission of the Ionized Exhalations of Phosphorus Through Air and Other Media. C. BARUS 257
An Hermetically Sealed Type of Clark Standard Cell. H. T. BARNES . . . 268
Concerning Thermal Conductivity in Iron. EDWIN H. HALL 277
Spark-Length of an Electric Influence Machine as Modified by a Small Spark from the Negative Side. W. J. HUMPHREYS 311
Index 317

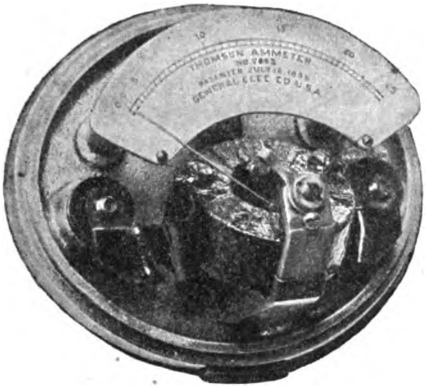
The

General Electric Company's

Pocket Ammeters

INCLINED COIL POCKET AMMETER (Covered)

of twenty-five amperes' capacity can be used upon either direct or alternating current, and gives accurate readings for experimental work in the laboratory.



INCLINED COIL POCKET AMMETER (Uncovered)

This pocket ammeter is one of a complete set of portable inclined coil electrical measuring instruments, for use in the laboratory, central station and outside work.

GENERAL ELECTRIC COMPANY

Main Office: SCHENECTADY, N. Y.

Sales Offices in All Large Cities

For CANADA: Address the CANADIAN GENERAL ELECTRIC COMPANY, Toronto, Can.

WM. GAERTNER & CO.

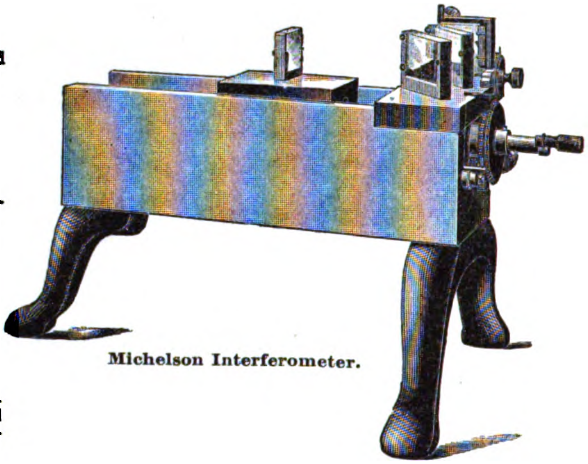
Astronomical & Physical Apparatus

152-158 E. 56th Street, CHICAGO

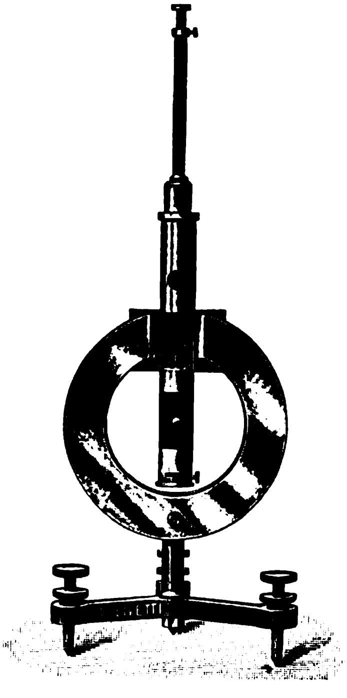
SPECIALTIES

- Standard Apparatus of new and improved designs
- Astronomical Telescopes
- Spectroscopes
- Micrometers
- Michelson Interferometers
- Reading-microscopes and Telescopes
- Bolometers
- Heliostats
- Dividing Engines, Comparators
- General Laboratory Apparatus
- Universal Laboratory-supports

Special attention paid to the construction of instruments for original investigation after our own or furnished designs.



Michelson Interferometer.



Professors and Assistants

in electrical laboratories don't like to give students fine galvanometers because such instruments are liable to be spoiled.

They are perfectly right

Students

don't like to work with cheaply made instruments, because they are not correct and are troublesome to handle.

They are perfectly right

Use Olivetti's Reflecting Galvanometers

and you will have high-grade up-to-date instruments which don't fear hard service.

Catalogues and prices for the asking

CAMILLO OLIVETTI

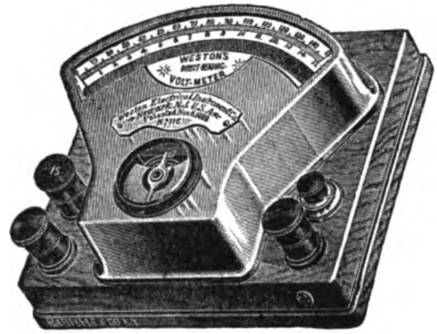
Maker of Electrical Instruments

IVREA, ITALY

WESTON

STANDARD
PORTABLE
DIRECT-READING

VOLTMETERS
MILLIVOLTMETERS
VOLTAMMETERS
AMMETERS
MILLAMMETERS
GROUND DETECTORS AND CIRCUIT TESTERS
WATTMETERS
OHMMETERS
PORTABLE GALVANOMETERS



WESTON STANDARD PORTABLE DIRECT-READING VOLTMETER FOR DIRECT CURRENT

Our Portable Instruments are recognized as The Standard the world over. The Semi-Portable Laboratory Standards are still better. Our Station Voltmeters and Ammeters are unsurpassed in point of extreme accuracy and lowest consumption of energy

---

Weston Electrical Instrument Co.

114-120 William St., NEWARK, N. J., U. S. A.

A History of Physics

In Its Elementary Branches, including the Evolution of Physical Laboratories

By FLORIAN CAJORI, Ph.D.

Professor of Physics in Colorado College.

Cloth Cr. 8vo, $1.60 *net.*

This brief popular History gives in broad outline the development of the science of physics from antiquity to the present time. It contains also a more complete statement than is found elsewhere of the evolution of physical laboratories in Europe and America. The book, while of interest to the general reader, is primarily intended for students and teachers of physics. The conviction is growing that, by a judicious introduction of historical matter, a science can be made more attractive. Moreover, the general view of the development of the human intellect which the history of a science affords is in itself stimulating and liberalizing.

"Prof. Cajori has followed his 'History of Mathematics' with a 'History of Physics' that is *even more interesting and valuable*. . . . Every teacher and student of physics should read it and add it to his library. It shows careful study, enthusiasm and a comprehensive grasp of the subject."—*Journal of Education.*

"A most valuable contribution to the subject."—*Scientific American.*

"Probably this will prove the most successful of all Prof. Cajori's histories."

—*The Nation.*

BY THE SAME AUTHOR

A History of Mathematics

Cloth 8vo, $3.50 *net.*

"What we have a right to expect in such a handbook is an agreeable narrative of the most material events in the history of mathematics, and this Professor Cajori incontestably supplies. The book was much wanted."—*The Nation.*

"A scholarship both wide and deep is manifest in this *History of Mathematics* which the author has infused with his own ardor in this department of science."

—*Journal of Education.*

"To the student with a love for mathematical science this book will be as entertaining as a romance."

—*The Transcript*, Boston.

A History of Elementary Mathematics

Cloth 12mo, $1.50 *net.*

"A most instructive, and at the same time a very readable piece of work, full of curious facts."—*The Bookman.*

"By no means an abridged edition of *The History of Mathematics*. It is an entirely new book, giving a somewhat detailed account of the rise and progress of Arithmetic, Algebra and Geometry. The book should be read by all teachers of these subjects, and by mathematical students generally."

—*American Mathematical Monthly.*

"The product of wide and scholarly research . . . For its historical facts and its suggestiveness, this work should be read carefully by all students and teachers of mathematics."—*The Dial*, Chicago.

THE MACMILLAN COMPANY, Publishers, New York

Recently Issued by The Macmillan Company

CAMPBELL—The Elements of Practical Astronomy
By W. W. CAMPBELL, Lick Observatory, California.
Cloth. 8vo. $2.00 *net.*
A new revised and considerably enlarged edition; comprehensive enough to include the principles which underlie all the varied work of a modern observatory.

CREW—The Elements of Physics
By HENRY CREW, PH.D., Professor of Physics in the Northwestern University. Cloth. 12mo. $1.10 *net.*
For the use of High Schools, etc.

HOSKINS—Elements of Graphical Statics
A TEXT-BOOK FOR STUDENTS OF ENGINEERING. By PROF. L. M. HOSKINS, Leland Stanford Jr. University. Illustrated. *New Edition Thoroughly Revised.* Cloth. 8vo. $2.25 *net.*
In scope, the work has been planned with reference to the requirements of students of engineering. This limits the development of the general theory to such principles and methods as are practically useful.
"The book is exactly what it purports to be—the elements of graphical statics. It is a well-written book for engineering students, the study of which will well repay them."—*Engineering and Mining Journal.*

WILLSON—Theoretical and Practical Graphics
An educational course in the theory and practical applications of descriptive geometry and mechanical drawing, prepared for students in general science, engineering or architecture. By FREDERICK NEWTON WILLSON, C.E. (Rensselaer), A.M. (Princeton), Professor of Descriptive Geometry, etc., Princeton University, Mem. Am. Soc. Mechanical Engineers. Complete. 4to. Cloth. $4.00 *net.*

Descriptive Geometry—Pure and Applied, with a chapter on Higher Plain Curves and the Helix; a theoretical and practical treatise prepared for courses in general science, engineering and architecture
Part VI of the above, $3.00 *net.*

Some Mathematical Curves and their Graphical Construction. A Brief Treatise on the Properties, Methods of Construction, and Practical Applications of Conic Sections, Trochoids, Link-motion Curves, Centroids, Spirals, the Helix, and other Important Curves.
Part III of the above, $1.50 *net.*

Shades, Shadows, and Linear Perspective. For Students of Engineering or Architecture, Professional Draughtsmen, etc.
Part V of the above, $1.00 *net.*

Practical Engineering Drawing and Third-angle Projection. For Students in Scientific, Technical, and Manual Training Schools, and for Engineering and Architectural Draughtsmen, Sheet Metal Workers, etc. By FREDERICK NEWTON WILLSON, C.E., A.M., Professor of Descriptive Geometry, Stereotomy, and Technical Drawing in the John C. Green School of Science, Princeton University. 4to. Cloth. pp. 178, with 270 illustrations and 65 alphabets, etc. Price, $2.80 *net.*

Send for the Announcement List, and the List of Books,
Issued in 1898, by

THE MACMILLAN COMPANY, New York

New Instruments for 1900

The New Queen Wall Pattern Galvanometer

is superior to existing types in FACILITY OF LEVELLING; ECONOMY OF SPACE; HIGH SENSIBILITY; SIMPLICITY OF DESIGN.

The New Queen Portable Photometer

Most Compact Photometer made. Can be adjusted for use in one minute. Gives highly accurate results.

The New Queen Standard Photometers and Photometric Accessories

The Queen Induction Coils

4-inch to 25-inch spark. In advance of any coils made in amount of energy obtained from the secondary.

OUR FACILITIES

have been greatly increased by the extension of our Factory and the enlargement of our Electrical Laboratory, which enables us to promise prompt service and satisfactory work.

Our New Pamphlets on Photometers, Cable Testing Apparatus, Portable Testing Set, X-Ray Apparatus, sent on application.

QUEEN & CO., INC.

J. G. GRAY, President

New York Branch
59 Fifth Avenue

1010 Chestnut St.
Philadelphia

Volume X. *January, 1900.* *Number 1.*

THE

PHYSICAL REVIEW.

A DETERMINATION OF THE NATURE OF THE ELECTRIC AND MAGNETIC QUANTITIES AND OF THE DENSITY AND ELASTICITY OF THE ETHER. I.

REGINALD AUBREY FESSENDEN.

PART I.

Introduction.
Dimensional Formulæ and Qualitative Mathematics.
Previous Work.
Résumé of some Earlier Papers.
Reduction of the Number of Possible Theories.

PART II.

The Critical Relation.
The Variation of Permeability with Magnetic Intensity.
The Solution.

PART III.

Corroborative evidence : A, Permeability ; B, Capacity.
Determination of the Density and Elasticity of the Ether.
Summary.
Conclusions.

APPENDICES.

A. Experimental work to determine the relation between H and μ.
B. Abstract of Previous work in search of the Fourth Equation.
C. Nomenclature, Symbols and Units.

PART I.

INTRODUCTION.

THE advance of science is not a continuous sequence of successes, with, day by day, verifying experiment hot following brilliant theory and completer theory advented by suggestive experiment. Here and there in the field are positions where the pause seems absolute and where the expert of a single tool, after a lifetime of unsuccessful toil, may dejectedly believe the years to have passed in profitless endeavor.

Yet the work is not wasted, it is only that what was looked for to appear as kinetic is stored as potential; in due time this will take the desired form, and add mightily to the velocity of the advance. The forging of new weapons of attack, the running of parallels, the exact determination of the difficulties to be overcome, are a part of the advance as much as is the actual motion over the field, and in the end cause no delay.

When Maxwell wrote the last word of his electromagnetic theory he had reached a point beyond which we have yet to pass. His theory has been confirmed by the experiments of Rowland, Lodge and Hertz; it has been broadened and generalized by the masterful and masterly labors of Heavyside; it has been elucidated by the acute and penetrating analysis of Poincaré; Helmholtz, Fitzgerald and other of the great physicists have aided in its development, and yet we have made no marked advance as regards our knowledge of the nature of the electric and magnetic quantities.

And this is the more remarkable when we consider the great number of striking discoveries which have been made since then. Even in the direction considered by Faraday as perhaps the most promising point of attack, that is the behavior of vacuum tubes, the magnificent results obtained by Crookes, Thomson, Lenard and Röntgen are as yet not capable of being utilized for our purpose.

There has been no lack of growth of increase of mathematical power. Our knowledge of vortex motion, for instance, has been brought, by the papers of Thomson, Kelvin, Fitzgerald and Hicks, to a point which but a few years ago could hardly have been hoped for. In addition, the genius of Heavyside has placed at our disposal an engine of surpassing strength for physical work, though perhaps at present there will be more of us to respectfully admire it than to attempt to use it.

It will be of interest to briefly review the present status.

We can conceive of the action of one quantity of electricity upon another quantity of electricity, of one quantity of magnetism upon another quantity of magnetism, of a moving quantity of electricity upon a quantity of magnetism, and of the motion of an electromagnetic wave, as taking place in the free ether, in space entirely devoid of matter. Under these circumstances we have no lack of perfectly satisfactory theories. The emission theory, the elastic solid theory, the rotational-elasticity theory, all these give complete solutions. It is only when we come to consider the effect of matter that we meet with any difficulty.

Corresponding to the particular theory which we may choose to adopt, we have to assume a theory of the constitution of matter, and no theory has as yet been proposed which does not break down at one or more points.

The theories may be divided into two great classes, those which make the virtual density of the ether the same in all media, which is equivalent to making the magnetic permeability the density of the ether, and those which make the virtual elasticity of the ether the same in all media, which is equivalent to making the specific inductive capacity the density of the ether.

In the first of these classes are the theories of Fresnel, Kelvin and Fitzgerald.[1] In the second class are the theories of Neumann, MacCullagh and Larmor. Several theories of each class have been treated of with great power by Heavyside in his Electro-Magnetic Theory.

The point where it is generally conceded the theories of the first type break down, is in satisfying the conditions of continuity at the interface of different media.[2]

The second type of theories escapes, as Larmor has shown, from this difficulty. But Heavyside has pointed out, in the case of some of the theories of this class, that a new difficulty arises, in that the form of the equation expressing the loss of energy in a conductor due to its resistance is incompatible with experiment. And this defect, it seems to me, must exist in all theories of this class, including the one so admirably developed by Larmor.

---

[1] On a Hydro-Dynamic Hypothesis as to Electro-Magnetic Action, Proc. Roy. Dublin Soc., March 25, 1899.

[2] In this connection it may be asked if in the case of iron the outermost layers of atoms are magnetic (Ewing's experiments on joints seem to show that they are not), and if it may not possibly be found that there is an absorption of light on reflection from a magnetized pole piece which is a function of the rotation impressed.

There is thus apparently nothing to choose between the two classes of theories. Each class seems capable, by proper development where this has not already been done, of expressing practically all of our experimental facts, and each breaks down at one single point.[1]

There is no obvious reason why the theories should have been restricted to these two classes, for whilst, as Maxwell pointed out, the ether certainly has inertia, it does not follow that either permeability or capacity is its density. The fact remains, however, that theories of these two types are the only ones which have given even approximately satisfactory results, and it will later be shown why this must be so.

The problem divides itself into two parts, the nature of the quantities involved, and their configuration. Of both of these we are entirely ignorant. A knowledge of the former means a full solution of the problem of the nature of electricity and magnetism, in the same sense, in which we say that we know the nature of sound when we have discovered that it is produced by waves in a medium.[2]

Knowing the nature of the electric and magnetic quantities, there is a choice of suitable configurations, any one of which will be entirely satisfactory. Since, however, the true configuration must also account for gravitation and inertia, it would be futile to develop the theory to any extent in connection with a configuration which did not at the same time give promise of results in this direction also.

In the present paper the nature of the electric and magnetic quantities will be determined, and a configuration will be indicated which may serve us for the present.[3]

---

[1] It is possible that each of the perfected theories of each type may represent an actual material universe, the two universes possibly interpenetrating each other. We, for the present at least, are concerned with but one, and which one has not yet been shown.

[2] We have of course no idea what a wave is; we figure it as motion in a certain way, but the ultimate motion may be something entirely different; it may, for instance, be some complicated form of rotary motion.

[3] The determination of a configuration which will explain, in addition, the nature of gravity, must be left to the future, as there appears to be at least two possible solutions and an additional independent physical relation must be discovered before we can say with certainty which is correct.

It will be shown that the number of electric and magnetic relations which can be expressed in terms of the electric, magnetic and mechanical quantities,[1] is not sufficient for a solution. There are four unknown quantities and but three independent phenomena. Hence some new relation must first be discovered.

This, it will be shown, is a relation between the magnetizing force, H and the magnetization, I, expressed by the formula

$$\frac{dI}{dH} = c\frac{I^2}{H^2}$$

or, expressed in words, "For all magnetic substances, the rate of variation of magnetization with magnetizing force is proportional to the square of the magnetic susceptibility."[2]

It is shown that this leads to a solution, which is confirmed by additional evidence in the shape of other new relations. From one of the latter, data are obtained from which a determination of the density and elasticity of the ether is made.

It thus happens that, exactly a century after the discovery of Volta, we are permitted a knowledge of the nature of the electric current.[3]

Dimensional Formulæ and Qualitative Mathematics.

Fourier's statement[4] with respect to dimensions is: "It must now be remarked that every undetermined magnitude or constant has one dimension proper to itself and that the terms of one and the same equation could not be compared if they had not the same exponent of dimensions. This consideration * * * is derived from primary notions on quantities, for which reason, in geometry and mechanics, and is the equivalent of the fundamental lemmæ which the Greeks have left us without proof."

---

[1] By mechanical quantities is meant quantities which are known functions of length, mass, and time, only.

[2] Or translated into the elastic problem, it behaves like rubber. See Molecular Physics, Franklin Inst. Proc., Feb., 1897.

[3] This paper was completed in July, 1898, but has been held for consideration until now. It originally contained a section on the methods of employing qualitative mathematics for the discovery of new phenomena, and the working of physical problems. On consideration this has been omitted as not being in a sufficiently developed state.

[4] Theory of Heat: Freeman's translation, p. 128.

Dimensional formulæ were used extensively by Maxwell, who says :[1] "When a given unit varies as the *n*th power of these (three fundamental) units, it is said to be of the *n*th dimensions as regards that unit." "In all dynamical sciences it is possible to define these units (of different kinds of quantities to be measured) in terms of the three fundamental units of length, mass and time."

In several instances, however, for example in treating of viscosity,[2] he suppresses one of the dimensions. In the case instanced he finds, after so doing, that the dimensions of viscosity, measured in the kinematic way, are L^2/T, whilst he has previously shown that measured according to the dynamic method its dimensions are M/LT. The former dimensions are arrived at by taking the unit of mass as the mass of unit volume of the substance considered.

This suppression of one or more of the fundamental dimensions in the formula for a quantity seems to me to be, whilst no doubt perfectly legitimate, (since we have a right to make what rules we find convenient, provided we are consistent in our use of them), a considerable departure from what I take to be Fourier's idea. For if we suppress dimensions, then, instead of Fourier's "one dimension proper to itself," a physical quantity may have an infinite number of dimensional formulæ, the one we use depending upon which units we choose and which of those chosen we suppress.

We find numerous instances of this dropping of dimensions elsewhere. For example, in Everett's book on C. G. S. units we have the following equation,[3]

$$M = L^3/T^2.$$

Whatever Fourier's conception really was, at the present time dimensions are generally treated as being merely an affair of units, any particular dimensions appearing or disappearing as we vary the units with which we work. It is evident that such a system, in which the formula for any particular quantity may be varied at pleasure, can be of little use as a method of mathematical investigation, and some of our most able mathematical physicists have expressed an opinion to that effect.

---

[1] Elect. and Mag., Vol. I., p. 2.

[2] Theory of Heat, p. 299.

[3] C. G. S. System of Units, p. 73.

J. J. Thomson says,[1] "It may be well to state at the outset that the dimensions of the electrical quantities are a matter of definition and depend entirely upon the system which we adopt." Also, "A theory of dimensions cannot tell us what electricity is; its object is merely to enable us to find the change in the numerical measure of a given charge of electricity or any other electrical quantity, when the units of length, mass and time are changed in any determinate way."

Webster is equally definite. To quote[2]: "The absolute dimensions of either factor are arbitrary. Attempts have been made to settle the real dimensions of x and μ, but they are evidently based upon misconceptions of the theory of dimensions."

The weight of authority is very greatly in support of the definition of dimensions as merely arbitrary formulæ, expressing the relation between certain quantities and the units we chose to work with.

There is, however, a branch of mathematics of great power and capable of extensive and important application in physics, more especially as a means of discovering new phenomena, in which the formula assigned to any quantity represents its ultimate nature, whatever that may be. Of course, this does not imply that we *know* what its ultimate nature is, but this does not invalidate results obtained by its means. ML^2 is motion of inertia, no matter what M is ultimately, and nothing which we may find out about mass can change this fact. M may contain three or four factors, but, still, the product of these factors, which we represent by M, will when multiplied by L^2 continue to represent moment of inertia.

In view of the opinions and definitions of the eminent naturalists quoted above as to the meaning of the term dimensions it would be improper for me to apply that term in any other sense. I shall therefore call the formula expressing the ultimate nature of a quantity its "quality," and this branch of mathematics "qualitative mathematics." This term seems suitable, for the method is used to make deductions with reference to the *qualities* of a physical entity, as distinguished from other branches which concern themselves with

---

[1] Elements of Elect. and Mag., p. 447.

[2] Theory of Elect. and Mag., p. 418.

quantity or direction. The term has a precedent in science, for we speak of quantitative and qualitative chemical analysis.

The difference between the dimensional formula and the qualitative formula or quality of a thing is that, according to the definition of the writers quoted above, the *dimensions* "are arbitrary," are "merely a matter of definition and depend entirely upon the system of units we adopt," whilst the *quality* is an expression of the absolute nature and never varies, no matter what system of units we adopt. For this to be true, no qualities must be suppressed.[1]

All the phenomena of physics with which we are acquainted deal with the following quantities only:[2]

Name.	**Symbol.**
1. Quantity of gravity,	G
2. Quantity of electricity,	Q
3. Quantity of magnetism,	P
4. Gravitational coefficient,	γ
5. Specific inductive capacity,	κ
6. Magnetic permeability,	μ
7. Mass,	M
8. Length,	L
9. Time,	T

The defining equations being

$$\frac{G^2}{\gamma} = \frac{Q^2}{\kappa} = \frac{P^2}{\mu} = \frac{ML^3}{T^2}.$$

Any physical phenomena as, for instance the vibration of a stretched string, may be concerned with all these quantities, for it may be so long and heavy that gravitational force may be appreciable, it may have an electric charge on it, and it may be magnetized and vibrating in a medium of a given permeability and specific

[1] As an example of the difference between dimensional and qualitative formula we may take the result above referred to as contained in Everett's C. G. S. Units, p. 73. It is there shown that by taking the unit of mass as that which produces unit acceleration at unit distance, the dimensions of mass become $M = L^3/T^2$. It is evident that so soon as we attempt to use these dimensions in other than gravitational problems we must get incorrect results owing to the limitations we have placed upon ourselves. In the qualitative system, however, M is always M, if we take M as a fundamental unit, and if we take any other unit M is always represented by terms which are equal to M, so that no matter what branch of physics we work in we always use the same formula.

[2] This statement is as incapable of proof, and for the same reasons, as the statement embodying the law of the conservation of energy.

inductive capacity. If these quantities were not all expressible in terms of one or more of three fundamental quantities we should need, therefore, no less than six distinct experiments to completely define each phenomenon. As it is, however, we need but three. From this also it follows that, if the phenomenon can exist whilst concerning but three of the above eight quantities, the effect of the introduction of the other quantities is merely to add terms of the same quality as the original one.[1]

As an example, let us take a bead on a string. Its time of vibration depends upon, amongst other things, its mass. If it is charged the effective mass is increased, as is shown by J. J. Thomson (Application of Dynamics to Physics and Chemistry, p. 32), by an amount $2/15(\mu Q^2/a)$ (Heavyside says ⅓), where Q is the charge, a the radius of the sphere, and μ the permeability of the medium. Here $\mu Q^2/a$ has the dimensions M. If the pendulum had a magnetic charge, there would be a term $P^2 \kappa/a$, and if gravity had also to be taken into account we should have a term $G^2 \kappa\mu/\gamma a$.

---

[1] A symmetrical system might also be made by introducing coefficients analogous to κ and μ into the equations connecting M and G, as was done by the writer (Elect. World, May 18, 1895). Also (as was done, *ibid.*) by introducing Ψ, the symbol for energy, and making it the third fundamental unit with L and T. For the present the above will be used.

In addition to these, however, we have terms which express configuration and terms which denote direction or operations. In brief, this branch of mathematics may be taken as a very precise logic.

As an example of a term depending upon configuration we may take the case of temperature. The quality of temperature, as may readily be shown, is

$$\theta = ML^2/T^2 o,$$

where θ represents the quality of temperature and o the quality of an atom, temperature being the amount of kinetic energy per atom. It will thus be seen that whenever we have a qualitative equation in which some physical quality is shown to depend upon temperature, that quality must also be a function of the atomic weight, atomic volume, atomic valency or atomic magnetic moment.

Here the term atom is defined as that configuration upon which depends the various properties which distinguish the atom from the surrounding ether. By configuration is meant not only the relative positions, but also the relative lengths and velocities of the different parts of the atom. When we know these, however, we can split o up into separate terms. Even at present we can do this to some extent. For instance, from Faradays's law of electrolysis we have $M/$ atomic weight $= Q/$ valency. From which we get atomic weight ÷ $T =$ valency, which is interesting in view of Hicks's work.

Previous Work.

Maxwell, Kelvin and other writers have given dimensional formulæ for the electric and magnetic quantities, in which the dimensions of x and μ were suppressed. So far as I am aware the complete dimensions or qualities in terms of L, M, T, x and μ were first given by Rücker, in the *Philosophical Magazine*, February, 1889.[1]

It is also pointed out in the above-mentioned paper that if the electric and magnetic quantities are of the same nature as those met with in mechanics, then, since the ratio of the dimensions in terms of x to those in terms of μ is a power of $x^{½}$, $\mu^{½}$, L and T and the two expressions must be identical, $x^{½}\,\mu^{½}$ must equal T/L, which is Maxwell's equation.

Lodge has given, in his Modern Views of Electricity, p. 702, 1889, expressions for the electric and magnetic quantities in terms of L, M, T, x and μ, and a further list, taking μ to be a density. He points out that this is an assumption, and refers to Fitzgerald's suggestion that both x and μ may have the same dimensions.

Williams (*Phil. Mag.*, Sept., 1892) published a very valuable paper, in which, making certain very natural assumptions, he arrived at the result that there are but two possible solutions, either μ is a density and x a compliancy, or vice versa.

The writer (*Electrical World*, May 18, 1895) showed that all the electrical and magnetic relations which had been exactly determined could be brought under three equations. From this it was deduced that, since there are four quantities concerned, an infinite number of theories was possible, all of which would be in agreement with the above mentioned relations. It was also pointed out that further experiments must be made to obtain a fourth equation.

Joubin (*Journal de Phys.*, Sept., 1896) came to the conclusion from an examination of the manner in which the exponents of L, M, T, x and μ vary when the electrical and magnetic quantities are written in terms of them, that μ must be a density and x a compliancy.

There are considerable gaps in the reasoning by which this result is reached. After reducing the number of possible theories to twelve, the author remarks that they are all impossible with the exception of the one which makes μ a density. No reason is given for this statement, which moreover is incorrect.

---

[1] Though the fact that κ and μ should be included in the dimensions of these quantities had previously been pointed out, as I learn from Professor Rücker, by other physicists.

The writer (*Electrical World*, Jan. 2, 1897) pointed out that the most promising field of search for the fourth equation was amongst the following:

1. Hall's effect. 2. Kerr's electrostatic effects. 3. Fizeau's experiment. 4. Magnetic rotation of light and other outlying phenomena.

It was also pointed out that our knowledge of these phenomena is not at present sufficient to enable us to use them to obtain the desired equation.

Joubin (*Journal de Phys.*, Feb., 1897), following the method described in the paper just referred to, though apparently without knowledge of it, or of the warning given as to the risk of applying the method without further experiments, by considering certain formulæ which he takes as representing the phenomena of thermo-electricity, magnetic rotation of light and piezo-electricity, arrives at the conclusion that the result reached in his former paper is correct. There is however, little justification for the formulæ adopted to represent the phenomena considered, and with very little change the arguments advanced might be used to prove that it is κ which is the density.

Abstracts from Some Earlier Papers.

In the Electrical World, May 18, 1895, the writer pointed out that all the dynamical relations which have been accurately formulated connecting the electric and magnetic quantities are contained in the first three of the following equations:

$$Q/P = \kappa^{1/2}/\mu^{1/2}, \tag{1}$$

$$QP = ML^2/T, \tag{2}$$

$$\kappa^{1/2}\mu^{1/2} = T/L, \tag{3}$$

$$\kappa^{1/2}/\mu^{1/2} = Z. \tag{4}$$

The first equation is obtained from Coulomb's law,

$$QQ'/L^2\kappa = ML/T^2$$

and the corresponding magnetic formula

$$PP'/L^2\mu = ML/T^2.$$

The second equation is from Faraday's law, that the time rate of change of induction $P/7$, is a measure of the voltage, voltage multiplied by current giving power, *i. e.*,

$$P/T \times Q/T = ML^2/T^3.$$

The third equation is obtained from Weber's law, that a circuit carrying a current is equivalent in its magnetic effect to a magnetic shell whose moment is equal to the area enclosed by the contour of the circuit, *i. e.*,

$$Q/T \times \mu \times L^2 = PL,$$

by substituting for Q/P its value in terms of x and μ, as given from the first equation.

This third equation is known as Maxwell's equation.

It will be seen that we have four unknown quantities, Q, P, x and μ, whilst we have but three equations. Put $x^{1/2}/\mu^{1/2} = Z$ for a fourth equation, and solve—we get,

$$Q = M^{1/2}LZ^{1/2}/T^{1/2}, \tag{5}$$

$$P = M^{1/2}L/T^{1/2}Z^{1/2}, \tag{6}$$

$$x = TZ/L, \tag{7}$$

$$\mu = T/LZ. \tag{8}$$

By giving different values to Z we obtain all possible theories of electricity, *i. e.*, all theories which do not conflict with the known laws of electricity and magnetism.[1]

For example, let $Z = L^2T/M$.

On substituting we get

$$\begin{aligned} Q &= L^2 & F &= M/LT^2 \\ P &= M/T & H &= L/T \\ x &= LT^2/M & \mu &= M/L^3 \end{aligned}$$

where F is the drop in electrical potential per cm. or the voltivity, and H is the drop in magnetic potential per cm. or the gilbertivity.

---

[1] So far as the ether is concerned any one of these theories is sufficient. It is only when we come to consider matter as well that we need a fourth equation.

This theory will probably be most familiar from Lodge's provisional use of it in his Modern Views of Electricity, as a means of giving a most lucid and satisfactory working conception of most of the phenomena of electricity and magnetism.

If we let $Z = M/L^2T$ we get the reciprocal theory to the above, *i. e.*, that in which x is the density and μ the compliancy.

A very considerable number of theories has been treated of by Heavyside in his Electromagnetic Theory, Vol. I. That on page 254 may be obtained by letting $Z = M/L^2T$, and is thus the reciprocal theory referred to above, making x the density and μ the compliancy, though this might not appear to be so at first glance, as that titanic mathematician is fond of using a special notation. The other difficulties which he refers to in connection with this theory, which in a modified form was first suggested by Kelvin, are no doubt those connected with the discontinuity of electric force at an interface.

Let $Z = ML^2T$, then,

$Q = ML^2 =$ moment of inertia,
$P = 1/T =$ angular velocity.
$E = 1/T^2 =$ angular acceleration.
$G = ML/T =$ angular momentum.
$\mu = 1/ML =$ reciprocal of moment of inertia.

We can get an idea of the mechanism of this theory by considering the motion of a pulley loosely fixed on a revolving shaft.

Let $Z = T^3/M$, then

$$Q = LT, \qquad P = ML/T^2,$$
$$x = T^4/ML, \qquad \mu = M/LT^2.$$

This theory makes the force with which an elastic string is pulled analogous to quantity of magnetism, the amount of stretch analogous to the gilbertance[1] and the elasticity of the string analogous to the magnetic permeability.

Reduction of the Number of Possible Theories.

So far we have not limited the values of Z in any way. It may contain any power, integral or fractional, of M, L and T.

---

[1] Gilbertance, difference of magnetic potential.

But from examination of equations 5–8 we see that if Z does not contain M, then Q and P cannot be quantities of the same nature as those met with in mechanics, but must be square roots of such quantities. But if we have two charged bodies in space, having equal charges, the bodies themselves consisting of elastic hollow conductors, there being an indefinitely small hole in one of them, if we let the other conductor shrink so that it can be introduced into the indefinitely small hole in the other, the one conductor will now have a double charge. If these conductors are by themselves in space, it is difficult to see why, if Q were the square root of some dynamic quantity, the charge should be twice the original charge.

The same reasoning applies to the supposition that Q or P may be some other power than the first of one of the quantities we meet with in mechanics, and, hence, we arrive at the result that Q and P either contain M to the first power or else M is absent. This makes Z to contain M to the power $+1$ or -1, as may be seen from equation 5.

As regards the power of L, it is difficult, as Williams has pointed out, to understand how a meaning can be attached to powers of L higher than the third. L is a length, L^2 is an area, and L^3 is a volume, but L^4, in conjunction with M to the first power, is not at present understandable. Consequently we may for the present suppose that L does not enter into the qualities of Q, P, x and μ to a power higher than the third. From examination of equations 5–8 we see that this means that L must enter into Z either as the $0 + 2$ or -2 power.

But L cannot enter into Z as the 0 power, for since the capacity of a condenser has the quality xL and the permeance of a magnetic circuit the quality μL, either capacity or permeance must have the quality $M^x T^y L^0$, *i. e.*, must be entirely independent upon configuration. But we know that this is not true, for by changing the ratio of area to cross section we can change the capacitance or permeance and for a given change of ratio, the capacity is modified in the same way no matter what the substance or physical state. Consequently Z must contain L to the power $+2$ or -2.

Again, from inspecting equations 5–8 we see that the exponent of M must have the opposite sign to the exponent for L, otherwise either capacitance or permeance will have the quality ML^3/T^4 and either Q or P will have no dimensions, T having the index given below.

As regards the exponent of T, in order that the quality of Q or P may not contain a fractional power of T, the index of T must be odd. If we reject all powers of T above the fourth as improbable, then T must enter into Z with the index $+1$ or -1. For if it had the index $+3$ or -3, then either the time rate of change of current or of voltage would be the fifth differential with respect to time of some quantity. This may, of course, be true, but can hardly be considered probable.

As we do not, in mechanics, meet with any quantity in which the exponent of M is of the same sign as the index of T, except when they are both zero, we finally arrive at the result that the quantity of Z is

$$M^{\pm 1}L^{\pm 2}T^{\mp 1}$$

or, substituting in equations 7 and 8,
either x is a density and μ a compliancy, or vice versa.
a result which is identical with that of Williams, though derived in a different manner.

It will be observed that in reaching this result we have not assumed *a priori*, that Q and P contain M to the 1 or 0 power, but have taken the additive property of Q and of P as our reason for considering such to be the case. To the writer this seems conclusive, but as will be seen later, the above proof is not at all essential to my reasoning, though it happens to agree with it, and I am myself persuaded of its validity.

PART II.

The Critical Relation.

We need a fourth equation. An account of some of the work done in past years, with a view to obtaining this equation, is given in an appendix. Instead, however, of examining phenomena, to see if they furnish the required equation, we may employ the converse method, *i. e.*, examine what equations are possible and see if there are in existence phenomena which fit them. This is advisable for another reason. It is held by some, to whom I have explained my ideas, that phenomena cannot be predicted by means of qualitative mathematics. I might reply to this that the method has as a matter of fact, been successfully used for this purpose, but for the present I prefer to abandon that ground and to rest my argument on the fact that, whether phenomena can or cannot be predicted by means of qualities, yet when the phenomena are discovered, how or by what means it matters not, the qualities can be determined from them. In equations 1–3 we have relations between Q and P, and x and μ, but not between Q or x and P or μ. Now the relations between Q and x and P and μ involve P and Q respectively, giving merely the relations between the forces, fluxes and resistances. Consequently, we are thus naturally led to look for relations between Q and μ or P and x.

As Q and μ belong to different systems, we must reduce them to the same. This we do by noting that Q/T is the current and that this is a measure of the gilbertance. But μ is a specific quantity, consequently we must take the gilbertance per unit length, or the gilbertivity, *i. e.*, $Q/LT = H$. Similarly $P/LT = F$ the voltivity.

We are, therefore, to look for a relation between H and μ or F and x.

We may write F, H, x and μ in terms of M, L, T and Z, and by giving different values to Z, see what relations should exist if that value of Z were correct. This was done and many relations were examined, and the result was again reached, by methods which I am not prepared to defend, that, as either x or μ must be a density. For the present, assuming that the previous work of Williams and the writer is correct and that Z is either M/L^2T or L^2T/M, we obtain as a result, either

$$F = L/T \qquad x = M/L^3$$
$$H = M/LT^2 \qquad \mu = LT^2/M$$

or

$$F = M/LT^2 \qquad x = LT^2/M$$
$$H = L/T \qquad \mu = M/L^3.$$

From this we arrive at the conclusion,

Of $\varkappa$ and μ, the one which is a compliancy will be an inverse function of the corresponding force.

This is our touchstone, this relation. We know at once that the capacity of a condensor does not decrease when we increase the voltage on it, whilst we know that the permeance of a magnetic circuit does decrease when we increase the gilbertance. It remains to be proven that the rate of this change is that called for by the equation.

The Variation of μ with H.

The existence of a relation between H and μ has long been noticed, and a number of attempts has been made to formulate it exactly. Until recently the most satisfactory formula was that proposed by Lamont and Frölich. This is

$$1/\mu = a + bH.$$

This however is obviously incorrect for high values of induction, as it makes the total induction reach a limit, and Ewing's experiments show that this is not the case. A satisfactory formula was however proposed by Kennelly.[1]

He divides the total inductivity B, into two parts. One part due to the ether in the space occupied by the iron, and numerically equal to H which we will call B_e. This,[2] since the value of μ for the ether may be taken as 1, can increase indefinitely as H increases. The other part of B, that due to the iron, which we will denote by B_i, he calls the ferric inductivity. The ratio between H and B_i he calls the ferric reluctivity, and we will denote it by ν_i.

Then, with these definitions, Kennelly's formula is

$$\nu_i = 1/(\mu - \mu_e) = a + bH (= \text{numerically } 1/(\mu - 1).)$$

That this formula is correct within the limits of experimental error has been shown by Kennelly himself for a number of magnetic materials.

Figures 1, 2 and 3 show the relation between ν_i and H for various samples of iron, nickel and cobalt. It will be seen that in every case the main part of the curve is a straight line, as it should be.

---

[1] Trans. Am. Inst. E. E., Oct. 27, 1891.

[2] I shall, to avoid considerable confusion and the use of a large number of additional symbols, take μ and κ as unity in vacuo.

This formula, it will be observed, does not hold for the first part of the curve. There is an abrupt bend at low values of *H*.

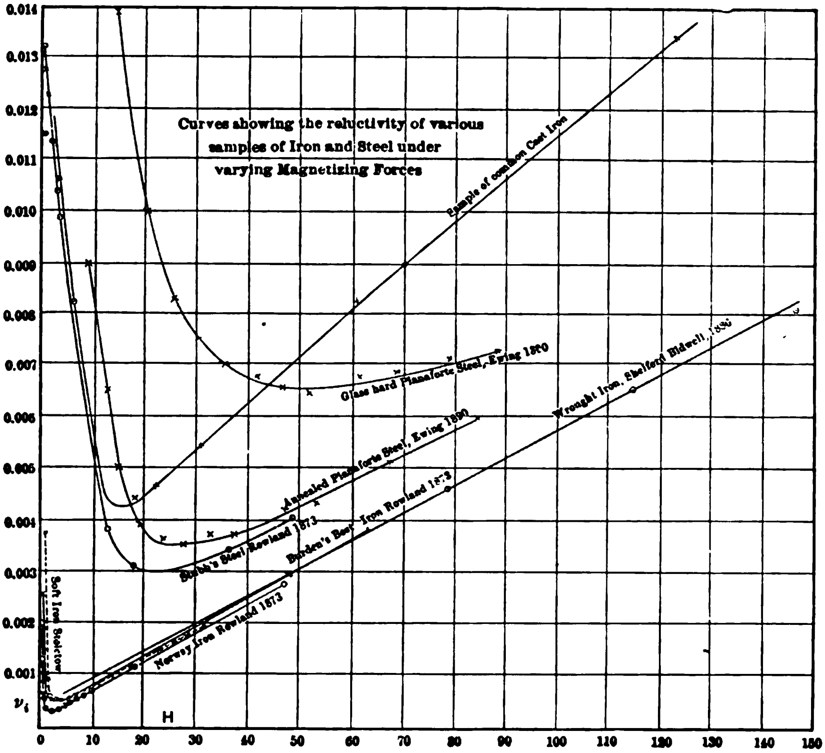


Fig. 1.

This bend however is not an essential part of the phenomenon; it disappears when the wire is tapped or has an alternating current passed through it. It seems to be due to the same cause as hysteresis, *i. e.*, molecular friction. It will be noted that those materials for which the hysteresis has a high value show the bend most pronouncedly.

Curve *A*, Fig. 5, shows the bend for a soft iron wire, and curve *B* is for the same specimen when an alternating current is passed through the wire. It will be seen that in the latter case the bend has entirely disappeared and the curve follows the formula throughout its length. The bend is therefore merely a superimposed phenomenon. The experiments from which these curves were drawn are those of Gerosa and Funzi.[1]

The question may, however, be asked, and has been asked, "Is not this relation one peculiar to iron and the other strongly magnetic metals?" This answer is, that it is not, but appears to hold for all materials. For Steinmetz has shown,[2] that the same law holds for alloys of iron, such as the non-magnetic 10 % amalgam of iron for which the permeability is as low as 2. As, however, these alloys all had a strongly magnetic component, the writer decided to investigate the magnetic relations of some of the very slightly magnetic materials. Fortunately, however, in view of the great dif-

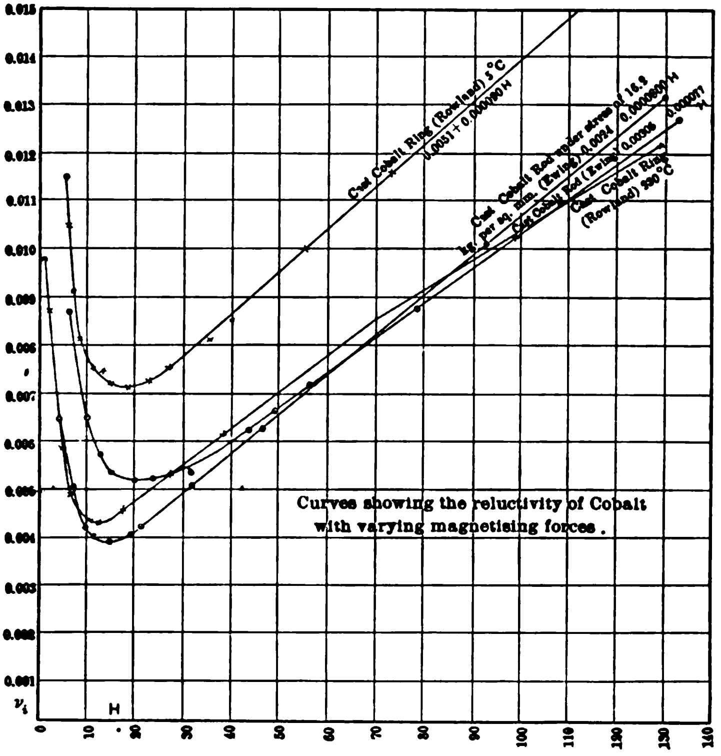


Fig. 2.

---

[1] Recond. del. Inst. Lombardo. See Ewing's "Magnetism in Iron and Other Metals," p. 319.

[2] Trans. Am. Inst. E. E., Sept. 27, 1892.

ficulty of the task, it was discovered that Silow had published some tests made on a solution of chloride of iron[1] and on plotting his results it was found that this substance also showed the same phenomenon, *i. e.*, the reluctivity decreases at first, but as *H* increases still further the reluctivity increases also. The subjoined table gives Silow's results, and it will be seen that they are of the same general nature as those obtained for iron though, possibly on account of the difficulties of the experiment, the curve is rather irregular. I propose to investigate this subject further.

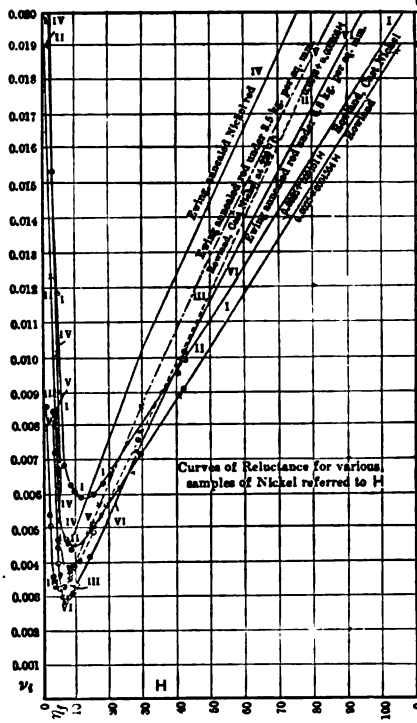


Fig. 3.

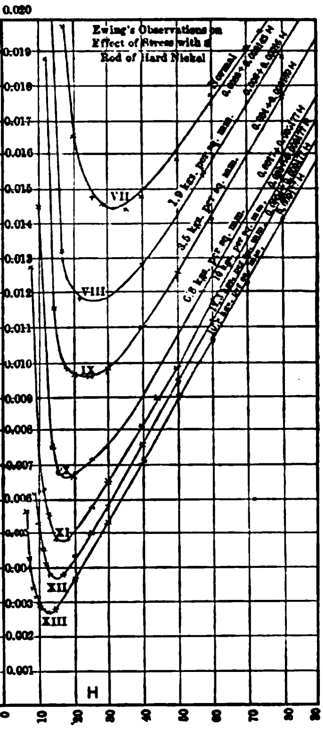


Fig. 4.

Plucker also noticed the same thing in the case of bismuth. A set of measurements, ranging from $H = 1100$ and up by another experimenter did not show the effect, but the test should be made with values of *H* from 1 to 20, as even in iron the permeability is practically constant when *H* is large.

---

[1] Wiedemann, Annal., Vol. XI., 1880, p. 324.

Table Showing Variation of Susceptibility with H, for Ferric Chloride Solution.

H	Susceptibility.	H	Susceptibility.	H	Susceptibility.	H	Susceptibility.
1.15	0.000096	1.81	0.000142	2.40	0.000099	6.54	0.000065
1.35	0.000104	1.90	141	2.45	104	7.00	062
1.60	131	1.96	131	3.73	070	10.00	060
1.70	131	2.13	111	5.33	069	12.60	055

As the relation is shown to hold for substances for which ν_i has such different values as 0.003 (iron), 0.004 (hard steel), 0.1 (iron filings)[1] 0.9 (11 % iron amalgam) and 1000 (ferric chloride solution), it is extremely probable that the law holds generally, and that we are justified in making the following statement:

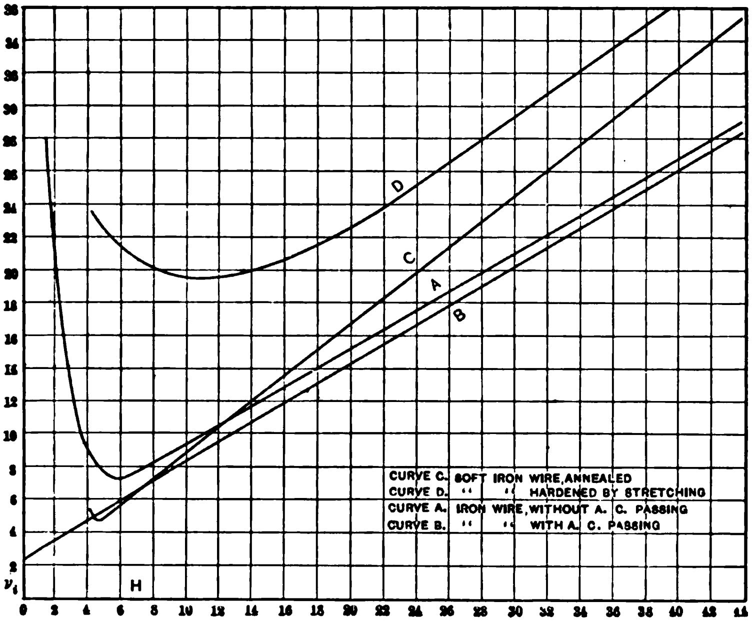


Fig. 5.

The relation between H and ν_i is given for all substances by the formula

$$\nu_i = a + bH$$

or, in a different form.

---

[1] See Steinmetz, ibid.

For all substances,

$$dB_i/dH = aB_i^2/H,$$

where, as will be seen later, *a* is a constant connected with the elastic properties of the material.

Having shown that the initial bend of the curve can be removed, and is thus merely a superimposed phenomenon, and that the law holds, in all probability, for all classes of substances, the question next arises, "Is the relation on exact one?"

Unfortunately we have at present no record of any very exact tests on iron or similar materials. That is to say, not exact as regards the connection between *B* and *H*, though no doubt amply precise enough for the purpose for which the tests were made. This is for the following reasons:

The experiments have been made on either straight wires, rings of iron wire, solid rings, solid rods or laminated rings. In all cases there are sources of error which were not taken into account in the experiments.

In the case of straight wires, the permeability varies with the induction, consequently the leakage varies also. This may give rise to errors of as much as 20% for high magnetizations.

In the case of rings of iron wire, no accurate results can be obtained, as the flux, having to pass from turn to turn through air, varies very greatly in density and the reluctance of the air spaces cannot be calculated owing to the varying permeability of the iron. Steinmetz has shown[1] that the variation from the mean induction in iron wire, wound somewhat differently, may be so great as to increase the hysteresis 900%. Such tests are not in any way a measure of the quantities supposed to be involved.

In the case of solid iron rods the induction cannot die away quickly owing to the eddy currents set up. Another still more serious error, especially with very thick rings, is the fact that the length of the magnetic circuit on the inside of the ring is less than that of the outside of the ring. If the ring is one foot in diameter, and the ring one inch thick, then this difference will amount to 16%. In other words the value of *H* in the inside of the ring is 16%

---

[1] Trans. Am. Inst. E. E., pp. 699 and 701.

greater than that in the outside. As the permeability is not constant, the mean permeability is not the same as the permeability of the mean circumference, and this difference may amount to several per cent. Though this source of error was pointed out by Kennelly[1] I know of no published experiments in which this very important source of error has been taken into account.

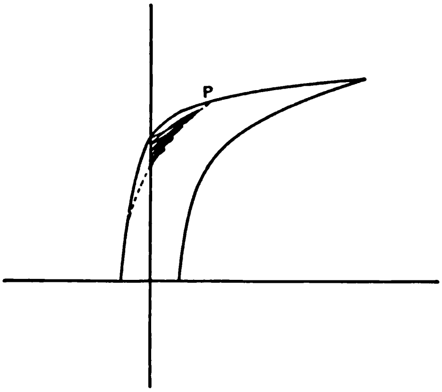


Fig. 6.

With solid iron bars tested in a permeameter there are quite a number of sources of error. But there is one thing alone which would vitiate all results obtained, especially for high values of B for which the permeameter is commonly supposed to give its most accurate readings, and that is that the formula used is incorrect.

The pull per sq. cm. is commonly given as

$$\text{Force} = (B^2 - H^2)8\pi.$$

It should be

$$B^2/8\pi\mu_m$$

where μ_m is the permeability of the medium, generally air, which flows in when the gap is made. The proof of this formula I have given elsewhere.[2] It is readily seen that the usual formula is wrong, for if we perform the following cycle:

1. Pull the bar, leaving the magnetizing coil in place.
2. Fasten the coil to the bar and let the bar come back to its original place.

---

[1] Electrodynamic Machinery, p. 28.

[2] Frank. Inst. Lecture, Magnetic Design, April, 1899.

3. Move the coil to its original place.

It is self evident that if the pull be given by $(B^2 - H^2)/8\pi$ we will have a perpetual motion machine.

It is for the above reasons that the writer was reluctantly forced to the conclusion that there was no published work which could be relied upon to settle the point whether iron obeyed Kennelly's formula with an accuracy of one per cent. or less. A very elaborate and careful test was then made, the results of which are shown in Fig. 7, the method being described in an appendix, and the conclusion reached was that:

The sample of soft iron tested obeys the formula, with a maximum deviation from it of less than ¼%, this being the maximum amount of experimental error, for values above the bend of the curve.

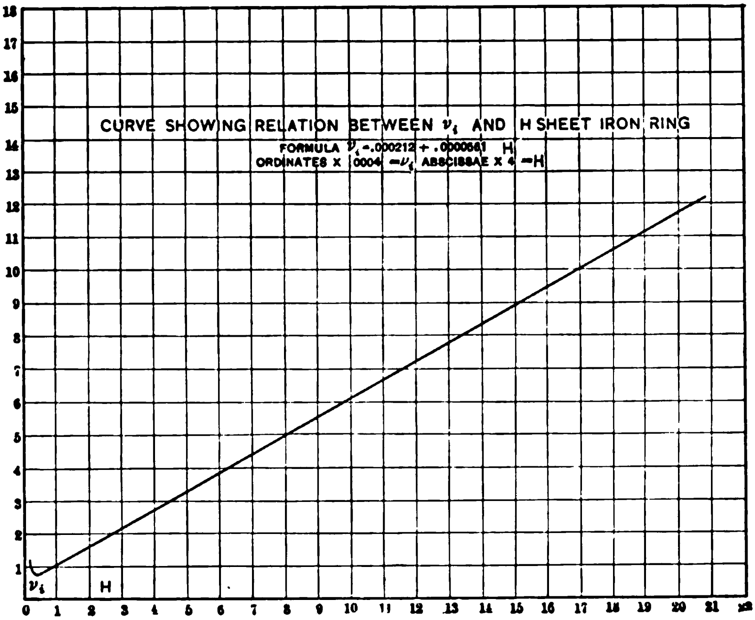


Fig. 7.

The Solution.

To resume, we have seen,

1. *That if the electrical quantities are of the same nature as those met with in mechanics, either x must be a density and μ a compliancy, or vica versa.*

2. *That, this being the case, either* x *must decrease as* F *increases, or* μ *decrease as* H *increases.*

3. *That whichever one of the two quantities,* x *or* μ, *decreases as the corresponding intensity increases, that one must be a compliancy.*

4. *That* x *does not decrease as* F *increases, but that* μ *does decrease as* H *increases.*

5. *That the rate of decrease is in agreement with the qualitative formula.*

6. *That therefore* μ *is of the nature of a compliancy.*

Z must therefore have the value M/L^2T. On substituting this value in equations 5–8 we obtain,

$$Q = M/T \tag{12.}$$

$$P = L^2 \tag{13.}$$

$$x = M/L^3 \tag{14.}$$

$$\mu = LT^2/M \tag{15.}$$

Part III.

Corroborative Evidence. A. Permeability.

Since, as we have seen

$$\nu_i = a + bH$$

and ν_i, being the reciprocal of a permeability, has the dimensions M/LT^2 then a must have the same quality.

Therefore a should vary with the elasticity of the material.[1] We can see that this is true by examining the curves of Fig. 1. Here the very hard materials, like pianoforte steel have large values of a. The soft materials, such as soft wrought iron have very low values of a. Intermediate materials, like cast iron have intermediate values of a.

Again if the material be hardened by stretching, a is increased. This is shown from the curves in Fig 5, taken from the experiments of Ewing.[2]

---

[1] This may be questioned. I will not defend my statement here, as the main matter of importance is the fact that it *does* vary, whether the prediction of the phenomenon was or was not legitimate.

[2] Ibid., p. 304.

It will be noted that though *a* has changed markedly, *b* remains unchanged.

Again, M/LT^2 is of the same quality as a stress. Consequently we should expect *a* to change when a stress is applied to a wire. The curves shown in Figs. 4 and 2 show that this is true for both nickel and cobalt. These are drawn from Ewing's experiments,[1] and are taken from Kennelly's paper.[2] That the same thing holds true for iron may be seen from Ewing's experiments on that substance[3] though the experimental data are not very numerous.

In some of the curves given, it would appear as if *b* also changed slightly. This may be true, but it may also be due to the fact that when the bend is very gradual it is difficult to calculate the value of *b* from the data given, as the line remains inflected for some distance past the bend. The direction of the change of *a* should be noted as it gives us an idea of the nature of the stress.

M/LT^2 has the same quality as η in Steinmetz's formula for hysteresis, for both have the quality of work per unit volume. We might therefore expect η to change with *a*. The accompanying table[4] shows that this is true. Note especially the cases where the same material is tested in the annealed and hardened states. In each case *a* and η change, whilst *b* remains constant.

b has zero dimensions, and I have found that there is a relation between *a*, *b* and η as follows,

$$\frac{b\eta}{a} = 0.007, \text{ approximately.}$$

There is some evidence in favor of the idea that *b* has but two values for iron, one for soft iron and one for cast iron, with intermediate values when the material is intermediate in quality. It may therefore be related to the chemical constitution of the sample, and may represent the reciprocal of the number of molecular groups. There is not sufficient data at present available to enable us to decide the question.

---

[1] Ibid.

[2] Ibid.

[3] Ibid., p. 179.

[4] Taken from Steinmetz's paper, Ibid.

Table Showing Relation between a, b and η for Various Substances.

Material.	a	β	η	$(\beta\eta/a)\times 10^6$
Wrought Iron, Norway	.166	.054	0.00228	742
" " Common	.20	.055	0.00326	896
Sheet Iron	.275	.058	0.0035	738
" " Ferrotype, Commercial	.45	.050	0.00548	609
" " " Annealed	.337	.050	0.00458	679
" " Tinplate	.192	.055	0.00286	819
" " "	.321	.053	0.00426	703
Very Soft Iron Wire, Erwing	.20	.064	0.0020	640
Cast Iron No. 1	2.40	.094	0.0130	509
" " " 2	2.43	.094	0.0132	510
" " " 3	2.76	.095	0.0158	544
" " " 4	2.05	.097	0.0113	535
" " " 5	2.34	.095	0.0127	516
" " " 6	2.07	.097	0.0122	572
" " " 7	2.37	.098	0.0136	562
" " " 8	2.92	.095	0.0146	475
Cast Steel, Hardened	2.7	.054	0.0280	560
" " Annealed	.88	.054	0.00848	520
" " "	2.00	.091	0.012	546
" " Average of 5 samples	.736	.057	0.009	679
" " " "	.35	.054	0.005	771
Mitis metal	.30	.054	0.00428	770
Welded Steel, Tool Steel HI	8.0	.121	0.0748	1133
" " " " H	7.8	.105	0.0613	825
" " " " OH	1.9	.066	0.0267	927
" " " " O	1.54	.060	0.0270	1055
" " " " Sh	1.33	.060	0.0190	857
" " " " S	1.22	.058	0.0145	689
" " " " Nickel	1.00	.170	0.0122	207

Corroborative Evidence. B. Specific Inductive Capacity.

Since μ is a compliancy, x must be a density and its quality must be M/L^3

The fact that dense substances are generally highly refracting was first pointed out by Newton. Diamond and Faraday's dense glass are examples, as are also those liquids used for making determinations of the densities of heavy minerals. The following table shows the relation plainly. The data are taken from Everett's C.G.S. Units, and Landolt and Bornstern's tables. The tests on the glasses were made by Hopkinson ; those on the gases by Ayrton and Perry.

Substance.	Density.	$\kappa - 1$.	$(\kappa - 1) / \text{Dens.}$
Glass.	4.5	9.1	2.0
Glass.	3.66	6.4	1.75
Diamond.	3.5	5.1	1.46
Glass.	3.2	5.85	1.82
Glass.	2.87	5.57	1.59
Sulphur.	2.	2.84	1.42
Benzol.	1.29	1.58	1.22
Rubber.	.92	1.32	1.43
CS_2.	.88	1.36	1.54
Paraffin.	.87	1.29	1.48
Kerosene.	.8	1.07	1.34
Petroleum Spirit.	.7	.92	1.31
SO_2.	.00269	.0052	1.93
CO_2.	.00195	.0023	1.18
Coal gas.	.00125	.0019	1.52
Air.	.00127	.0015	1.18

It will be seen that in the case of these substances, though the densities vary from 4.5 to 0.00125, or in the ratio of 4000 to 1, the added κ is roughly proportional to 1.5 times the density.[1]

As this table might give a wrong impression, it should be stated that though the bodies taken were chosen at random, yet several substances, such as water and the alcohols, which behave anomalously in other ways, have been omitted. Some of the discrepancies may be accounted for. Thus I have found that the capacity of most of the organic oils is considerably decreased when they are carefully purified from organic acids, mucins and water. In the case of hydrogen the discrepancy may be due to the difficulty of measuring such a very small capacity.

But we cannot expect this relation to be more than an approximation for the following reason: The value of κ varies with the periodicity and hence it is certain that, as in the case of the stretched string, the virtual mass must be made up of several terms. In fact, J. J. Thomson has shown that the capacity of a moving sphere is increased in the ratio $1/(1-4/15\,\mu\kappa V)$. So that in addition to the density, the expression for κ for a substance must contain terms depending upon the valency, the number of atoms per cc. the velocity of the atoms and the periodicity of the electric stress. These other terms may, it is conceivable in certain cases, be of larger numerical value than the original density term.

---

[1] Newton gives a similar table (Opticks, Prop. X.).

Viewed, however, not as an attempt to find a law for refraction, but merely as a broad test between two rival theories of electricity, it is evident that x is related to the density and not to the elasticity of the substances.

As regards the variation of x in any particular substance with change of state, we have Newton's, Gladstone's, Lorenz and Lorentz's and Ketteler's formulæ. The first three of these do not agree satisfactorily with experiment. The last one does, probably because it contains more constants.

The series in Ketteler's formula can be expressed as a fractional power, and hence is of unity dimensions. This makes the refractive index to vary as the density, and since μ is practically unity for all the substances to which this formula applies, we get $x =$ density. Not much importance can be attributed to this result however.[1]

It is to be noted that the refractive index of a gas seems to vary in the direct ratio as its density. But the denser the gas the closer the atoms must be to each other, and the greater the molecular attraction forces, according to Van der Waal's law (or the writer's modification of it, *i. e.*,

$$\left(p + \frac{c}{v^{\frac{4}{3}}}\right)(v - b) = R\theta\,).$$

Hence, also, in all probability, the greater the permeability. But, unfortunately, we know too little about these molecular actions at present to enable us to use the phenomenon in this connection.

Determination of the Density and Elasticity of the Ether.

If the ether has specific mass, and from the phenomena of light it must have some equivalent property, how would it affect the refraction of light?

In the first place we know that for most substances μ, the compliancy is practically the same as for the free ether. This means either that the elasticity of the free ether must be the same as that of the substance, or else that the total area of cross-section of the atoms is very small compared with the cross-section of the space they occupy to the exclusion of other atoms, *i. e.*, that the atoms have configuration virtually equivalent to that small framework structures of thin platinum wire interpenetrated by another substance.

---

[1] These considerations suggest the following formula: $\frac{n^2 + 1}{\text{density}} + A = \text{Constant}$, where A represents terms depending on the velocity, valency of the atoms, etc.

If we are to look upon the atoms as possessing a structure whose actual cross-section is small compared with that of the space occupied by them,[1] their effect on the elasticity and density constants would be analogous to that obtained by adding to a vessel filled with oil a number of small structures built out of platinum wire. In this case the cross-section of the oil not being appreciably changed, the elasticity would not be altered to any extent, whilst the density of the platinum wire structures would be added to that of the oil. The resultant velocity of wave propagation in the space occupied by the combination would therefore be given by the formula

$$v = R/(d + D)$$

where d is the density of the oil, D the mass of the platinum wire structures contained in one cc. of space, and R the elasticity of the oil.

In this case, if the oil were set in motion with a velocity v, the velocity of the waves through it would be increased by the amount

$$vD/(D + d).$$

This fraction is of the same form as that called for by the aberration of light and Fizeau's experiment. Consequently if the previous work be correct and x is a density, then the fraction $(x - x_0)/$ density,[2] should be constant for all substances, at least to a first approximation.

Now the above table shows that this is approximately the case. There are exceptions, such as water and the alcohols, but by far the greater majority of substances obey this law. We know so little about the constitution of the atom that we cannot at present discuss the peculiar behavior of the exceptions. It is to be noticed that those bodies which do not agree with Maxwell's[1] law, that the refractive index for light is $x^{\frac{1}{2}}\mu^{\frac{1}{2}}$, are those which also disagree most widely from this rule that $(x - x_0)/$ density is a constant.

---

[1] The preceding table was prepared before I noticed the great discrepancy between Ayrton and Perry's and Boltzmann and Klemencic's values of κ for gases. If the values given by the latter be correct, the atoms may be comparatively solid, and we obtain 1.33 as the density and 12.10^{20} as the elasticity of the ether. As this is not yet absolutely certain, I leave the above as written.

[2] Taking κ for the ether as unity, this is numerically equal to $(\kappa - 1)/$ density.

The determination of an approximate value of the density of the ether from this formula is a very simple matter. Since in the formula, x is the specific inductive capacity of, or density of, both the stuff and of the space occupied by it, and we take the specific inductive capacity of the ether, according to our present system of units, as unity, then $x - 1$ is the density of the material in terms of the density of the ether. The average value of the constant is 1.5, therefore the value of the density of the material in terms of the density of the ether is 1.5 times its density in terms of water, *i. e.*, the density of the ether is 0.66, or two-thirds that of water.

From this, by means of the formula for the velocity of light, we may deduce the elasticity of the ether and find it to be

$$R = 6 \times 10^{20}.$$

As a converse proposition, it follows that to find the specific inductive capacity of any substance, all we have to do is to add to its specific mass the specific mass of the ether, and then divide the sum by 0.66. Thus, to get the specific inductive capacity of a mineral oil of density 0.5, we get

$$(0.5 + 0.66) \div 0.66 = 1.75 = x.^{2}$$

It will be noted that the values given for the constants of the ether lie midway between those generally given. Lord Kelvin's value for the elasticity[3] is 900, with a correspondingly small density.

---

[1] I have previously pointed out that those substances which do not obey Maxwell's law act as negative uniaxial crystals in Kerr's experiment, whilst those obeying the law act as positive crystals, and that this indicates that those acting as negative crystals probably contain a certain per cent. of particles of substances of large specific induction capacity. —Elect. World, Jan. 2, 1897.

[2] Accepting Boltzmann's and Klemencic's results for κ we have, Mass of atoms in one c.c. — density of ether (1 — volume actually occupied by atoms) $= \kappa$. Hence, for spherical atoms not in the form of crystalline solids, we have the formula $(n^2 - 0.7)$/density = constant.

[3] Trans. R. S. E., XXI., 60.

This value, however, can hardly be correct. For unless the atoms hold together by means of hooks of some kind, it would seem as if the stress when a bar of metal is sheared must ultimately come on the ether. In fact, the writer has shown[1] that the phenomena of rigidity, elasticity and tensile strength are quite well explained by supposing that the atoms are electric doublets and have charges on them of the same size as those called for by Faraday's law of electrolysis, and that the effects observed are of the same dimensions as those called for by this theory. But whether this theory be correct or not, it would seem that if the ether is the thing ultimately acted upon, it must be at least as rigid as the most rigid substance we know of, *i. e.*, the diamond, which has a rigidity of approximately 10^{14}. This calls for a minimum density of the ether of about 10^{-7}, or approximately that of air at 1/15 mm. pressure. Therefore, on the very lowest supposition the density of the ether in a vacuum tube must be immensely greater than that of the enclosed gas.

The other hypothesis, that held by Fitzgerald and by some other eminent naturalists, makes the elasticity of the ether to be about 10^{21} and its density at least 20. At first sight it would appear extremely reasonable to suppose that, if the atom be simply some configuration of the ether, the ether must be at least as dense as the densest atom. More especially since, as has been pointed out, both the fact that the permeability, *i. e.*, elasticity of the ether, is but very little affected by the presence of matter and the fact that, (since the fraction $(x - x_e)/$density is approximately constant, *i. e.*) the density of the element is simply added to that of the ether, seem to show that the virtual cross section of the atoms is a small fraction (say one per cent. or less) of the space occupied by the atoms to the exclusion of other atoms. (Of course the actual space may be greater but in this case there must be some compensating device brought into play.)

But, on the other hand, it is not evident that there is any ***necessary*** connection between the density of the ether and the density of the atom. For it is very easy to see that if the atom consisted of some kind of a vortex in the ether, that owing to some property of the atom, say its energy of rotation, a strain might be set up in the ether proportional to its velocity squared. The total density of the atom might, therefore, be made up of two parts, one due to the density of the ether itself, which might be extremely small, and the other depending in some way on the configuration of the atom, this latter part being associated with a proportional quantity of gravity. Such an hypothesis, however small its justification, enables us to see that the ether *need* not be as dense as platinum.

---

[1] Elec. World, August 8 and 22, 1891, more fully, Science, July 22, 1892, and March 3, 1893.

Again, we may take a vortex ether, such as has been treated of so powerfully by Fitzgerald and Kelvin. In this case we may conceive of space as filled with a tangle of vortices, the actual volume of the vortices being small compared with the space they occupy, this tangle of vortices being what we call the ether. If, by any action a new configuration is made to take place, and this is done in such a manner that it has the properties of matter, it may, at the same time, call for a crowding together of the vortices at that portion of space. In fact there are a number of, not at present improbable, suppositions which we might make for the purpose of showing that there is no *necessary* connection between the density of the ether and of the elements.

(*To be continued.*)

POLARIZATION AND INTERNAL RESISTANCE OF THE COPPER VOLTAMETER.

BY B. E. MOORE.

MEASUREMENTS of electrolytic polarization may be divided into two general classes. In the first class may be placed all the measurements made with the direct current. This class includes the greater part of the determinations. In the second class the alternating current has been employed. The development of this method is a comparatively recent one, but promises to yield extremely important results. This method has also afforded measurements of electrode capacity, designated by "polarization capacity"—an expression which first appeared in a contribution by Kohlrausch.[1] This term suggests that the phenomenon is limited to the electrodes and to the solution immediately adjoining them. The enormous values obtained for the capacities,[2] indicate strata so thin as to be scarcely designated as finite. Their values are a reminder of the Helmholtz charged strata theory. However, the Warburg theory, which makes it a current conduction, and at the same time recognizes that both the electrodes and the solution play important parts, is more satisfactory. The process at the electrodes is frequently complicated. Gases may be deposited and cover parts of the electrodes and thus insulate parts of the surfaces, or the gases may be occluded and form an opposing gas battery. There may be an absorption of ions in the surface of the electrodes. The liberated products may form at the surface of the electrodes a new material or the same material in a different state of aggregation.

---

[1] F. Kohlrausch, Pogg. Ann., 148, p. 443, 1872.

[2] M. Wien, Wied. Ann., 58, p. 37, 1896.
C. M. Gordon, Wied. Ann., 61, p. 1, 1897.
A. M. Scott, Wied Ann., 67, p. 388, 1899.
E. Warburg, Wied. Ann., 67, p. 493, 1899.
E. Newmann, Wied. Ann., 67, p. 500, 1899.

The solution may act upon the electrodes either upon open or closed circuit—an action which may be normal or arise by reason of impurities, either in the electrodes or the solution. The character of the solution at the electrodes when carrying a current may be changed sufficiently to cause action. A concentration series always arises owing to the unequal rates of migration of the ions. Any of these changes give rise to a polarization. These deposits upon or in the electrodes are frequently small and a slight variation in their total quantity affects appreciably the electromotive force, when these deposits are present at the electrodes, before the current is started. Their ions are also present in the adjacent solution, and the current affects their magnitude; therefore, the concentration series in the solution is always affected. After removal of the current the electrodes and adjacent strata of the solution would tend to return to the former state of equilibrium, and the concentration series would be removed by diffusion. However the process of diffusion is a rather slow one and could only appear rapid when the concentration strata are minutely thin. If the current had suddenly been reversed, it must have aided the return to the original condition and quickly established the same phenomena as before, but at the opposite electrodes. This is what probably happens when alternating currents are used under proper conditions. This makes the phenomena reversible, and upon its reversibility depends the accuracy of the electrolytic resistance, by means of alternating currents.

The observations for electrolytic resistance by means of the direct current, unlike the observations by means of the alternating current, have never given uniform values. The resistance is always larger when the current is small. The method of measuring electrolytic resistance by direct current involves the measurement of three quantities, viz. electromotive force, E, at terminals of the electrolyte; the current, i; and the polarization, P. The electrolytic resistance, r, is then calculated by the formula $r = (E - P)/i$. The determination of E and i is easy and accurate. However, P is not usually measured with the direct current still flowing. The circuit is broken and the voltameter thrown upon an independent current. The process requires time and a residual part only of the polarization as measured. Wiedeburg has given an experimental formula[1]

$$P = \frac{P_1}{2}\left(1 - E^{-\frac{B_1}{S_1}q}\right) + \frac{P_2}{2}\left(1 - E^{-\frac{B_2}{S_2}q}\right)$$

where P is the total polarization. P and P' the maximum polarization at electrodes 1 and 2 respectively. s denotes surface of electrodes, B_1 and B_2 are constants depending upon the material of the electrodes, and q is the quantity of electricity which has passed.[2] Guthe has subjected the formula to an accurate experimental test. His experimental conditions made $P_1 = P_2 = P/2$, $B_1 = B_2$, $S_1 = S_2$ and the value of P so obtained, substituted in the simple formula above, gives the uniform values for electrolytic resistance. However, the actual measurement of P or an approximation to such a measurement is very desirable. As has been pointed out by several observers, the nearer the time from the opening the one circuit to the closing of the other approaches zero, the more accurate the values obtained for P, and therefore the more accurate the calculated value for r.

The object of the following experiments, is to emphasize these conditions and to show the growth of polarization upon closed and open circuit. It has therefore been necessary to employ the direct current. P has been measured between .00006 second, and .045 second after the polarizing electromotive force was removed. The latter has also been given wide ranges both in its value and in the time during which it was applied. The smallest time intervals, so far as could be learned, are ten times shorter than any previous experimenter has used.

Fig. 1 gives a diagram of the connections. The circuits were all made and broken by a swinging pendulum. The minute lateral motion of the pendulum was a troublesome factor until the steel cone pivots were drawn so tight that the pendulum ceased vibration after a few swings. The keys 1, 3 and 4 had micrometer screw attachments so that they could be adjusted accurately in the same plane as key 2. To obtain the deflection with the standard cell, B, key 1 is fastened open with a strip of glass, keys 2 and 4 are set, the six-point switch is turned to the left and the key *b* is closed by hand about the instant the pendulum is released. This was necessary since the time period from key 3 to key 4 or from key 1 to key 4 was not long enough to allow the condenser to assume the potential of the standard, owing to the large internal resistance of this cell. To obtain the total deflection from the storage cell the manipulation is the same except key 1 is used instead of key *b*. To polarize the voltameter key *a* is closed and keys 1, 2, 3, 4, 5 and 6 are set. When the pendulum is liberated, it first meets key 5 and opens the voltameter short-circuit. The detent to key 6 is overturned last of all and this again short-circuits the voltameter. By this swing of the pendulum the galvanometer will give a deflection proportional to the fall of the potential over the resistance *R* or the voltameter *V*, according as the six-point switch is turned to the left or right respectively. This fall of the potential over *V* will be the polarizing electromotive force, if keys 3 and 4 are released before key 2. It will be the polarization electromotive force if keys 3 and 4 are

---

[1] Wiedeburg, Wied. Ann., 51, p. 2, 302.

[2] Guthe, PHYS. REVIEW, Vol. VII., p. 193.

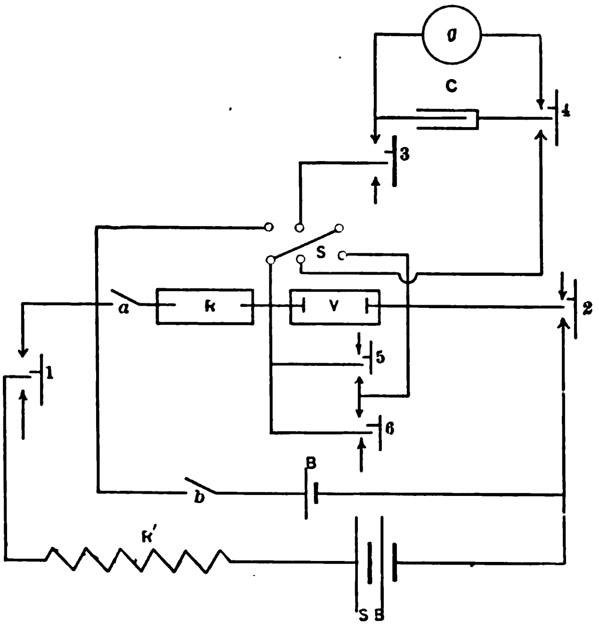


Fig. 1.

released after key 2. The deflections over R and V are also proportional to the resistance R and the apparent resistance R_v of the voltameter respectively. The deflection caused by the polarization of V, subtracted from the deflection produced by the applied electromotive force leaves a value proportional to the internal resistance r. Whenever deflections are thus compared it is necessary that the time intervals of charging be the same. For the apparent capacity of condensers vary when the time interval is very short.[1] The capacities of a one-third microfarad condenser and a one microfarad condenser were found to vary about four per cent. while a Stanley condenser varied three fold. Key 3 was placed so that it made the up contact after the detent to key 2 was released. The equation of motion of the pendulum and the distance key 4 was displaced from the plane of key 2, determines the time which has elapsed since the charging circuit was broken. To release keys 3 and 4 before key 2, a small hard rubber block was attached to the pendulum opposite keys 3 and 4. The current was determined from the total electromotive force of the storage battery and the total resistance of the circuit. When the currents were large it became necessary to measure the electromotive force of the storage cell upon closed circuit, since a concentration series[2] arises in that cell. A comparison of the deflections of the standard and storage cell gives the electromotive force of the storage cell. A deflection is then obtained from the storage when the time interval is short. Let this deflection be D'. Then, if d_r and d_v be the deflection on closed circuit over R and V when the condenser is charged the same length of time we have

$$(1) \qquad D' = \frac{(d_r + d_v)(R + R_r + R')}{R + R_v}$$

where D' is proportional to electromotive force of the storage cell, and R' the resistance in series with R and R_v. The internal resistance of the storage cell is neglected. Farther (2) $R_v = R\frac{d_v}{d_r}$. This value of D' differs slightly from D for large currents. However, this correction was only made in the first experiment, and for the others the electromotive force of the cell on open circuit was used.

---

[1] Physical Review, Vol. IV., p. 238.

[2] Physical Review, Vol. IV., p. 353.

The adjustment of keys 2, 3 and 4 for a very short time interval was an extremely tedious task. Keys 2 and 4 were down contacts and the best method to adjust them was found to be by the sense of touch. The detents were held with the fingers against the spring contacts and the pendulum drawn back through an arc of about 2 to 5 mm., and allowed to fall against them. The time sense in the touch of the fingers is so delicate, that, by this method, a distance of .007 mm. could be detected. When the pendulum swung through its full arc the time required to pass through this distance at the lower point of the arc was .00002 second. The smallest time between keys 2 and 4 actually used was .00006 second, and in this time interval there is liable to be an error as large as twenty per cent., in a few cases. This error arises from the inaccuracy of the adjustment, temperature changes in the keys, lateral vibration in the pendulum, and any displacement of the keys due to pressure in making the settings. This setting could not be made one day and be found in adjustment the day following. Neither could it be made and key 3 adjusted to fall with its up contact between keys 2 and 4. Key 3 with an up contact had to be adjusted electrically and after much use it was found that its position would depend upon the electromotive force employed. The adjustment of keys 2, 3 and 4 for such a time interval requires three hours' work and then the adjustment could not be expected to remain over one half hour. After long trial, this short time interval was abandoned. A trial made it about .0005 second, revealed the fact that key 3 could be dispensed with so far as the polarization phenomena were concerned. The readings were the same, after allowing for difference in time interval of charging the condenser through the external resistance *R*, when key 3 was set as when it was allowed to remain against its up contact. In the latter case the condenser becomes charged to the polarizing electromotive force, and then upon open circuit discharges through the voltameter until it reaches the polarization electromotive force. This discharge would tend to maintain the polarization if the quantity is an appreciable part of the "polarization capacity" of the voltameter. Farther the electromotive force of the condenser would not have fallen to the polarization electromotive force if the time interval was sufficiently short. This time interval must be determined from the equation of discharge of a condenser $q = \left(Qe^{-\frac{t}{RC}}\right)$ or $d = \left(De^{-\frac{t}{RC}}\right)$ where D is the total deflection of a galvanometer produced by a given electromotive force. E applied to a condenser of capacity C, and d the deflection of the same galvanometer after the condenser charged to the same electromotive force has been allowed to leak through a resistance R for the time interval t. A test of this kind was applied by charging the condenser over the resistance R made approximately equal to the resistance of the voltameter, and discharging it after leaking for an interval t. It was possible in this way to adjust the keys 3 and 4 so as to obtain an appreciable deflection, d. It was, however, not possible to obtain a deflection when the time interval was as long as .00006 second, provided the electromotive force of polarization was small. When the electromotive was large the spring in .00006 second is not sufficiently far removed from the lower contact to break the primary circuit. This process therefore became an excellent independent method to use as a check upon the settings of the keys. When the condenser is charged by the polarization electromotive force, after the polarizing electromotive force is removed, it lowers the potential of the polarization electromotive force, if the charge in the condenser is an appreciable part of the total charge in the voltameter, according to the law of divided capacities. For the time periods longer than .0006 second, a difference of two per cent. could be accounted for by experimental errors, but if there had been a difference of four per cent. in the readings, it could have been stated with some assurance that the capacity of the voltameter was between twenty-five and one hundred times as great as the capacity of the condenser. No such difference was detected and key 3 was allowed to remain against the up contact for all the recorded measurements and was only set when it was desired to make a test for capacity. No such effect was ever found with certainty but it seems reasonable to anticipate capacity measurements when the surface of the electrodes is reduced considerable in magnitude. After omitting the setting of key 3 the adjustment of keys 2 and 4 was greatly facilitated, and it became again possible to set to .00006 second, when the electromotive force was small. Wires not shown in the figure also lead from the voltameter to an alternating current bridge where the electrolytic resistance was determined by the Kohlrausch method.

The density of the solution used was 1.1395 throughout the experiments. The evaporation from the surface was compensated by adding distilled water. The temperature varied about four degrees in the first experiment, but less in the others. All solutions, except the first, contained about one-tenth per cent. of alcohol. The object of the alcohol was to prevent oxidation of the electrodes, but in this proportion it was not a success. The solution was also boiled to free it of the contained oxygen. Fresh solution from a stock bottle was taken for each experiment. The results of the first experiment are contained in Tables I. and II., and Figures 2, 3 and 4. The copper plates had stood in the solution for an entire week during preliminary tests, and the experiment itself continued through another week. At the end of this interval the plates were

TABLE I.

i.	*E.*	R_r	*i.*	*E.*	R_r
.00188	.106	56.44	.0274	.701	25.6
.003575	.236	66.	.0506	.9973	19.71
.00906	.435	48.	.0817	1.147	14.04
.01289	.5143	39.9	.1097	1.234	12.5
.01969	.6773	34.4	.2118	1.3765	6.5

TABLE II.

T	$E = .236$		$E = .4349$		$E = .701$		$E = 1.147$	
	P	r	P	r	P	r	P	r
.00006			.2176	24.	.3570	12.6		
.00018	.0898	40.6	.136	33.			.2973	10.4
.000295					.2352	17.		
.00033	.0512	5.7	.0671	40.6				
.00053			.0634	41.	.1753		.1911	11.7
.00073			.0544	42.		19.2		
.00090	.285	58.						
.00100			.0364	44.				
.00680	.0215	60.	.0309	44.6	.1366	20.6	.1666	12.
.01400	.018	61.	.0245	45.3			.1503	12.2
.0800			.0182	46.				

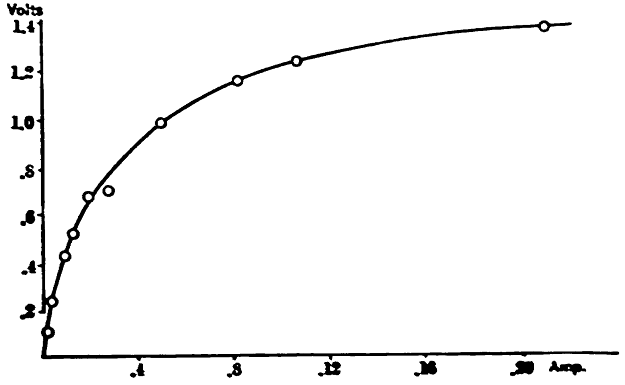


Fig. 2.

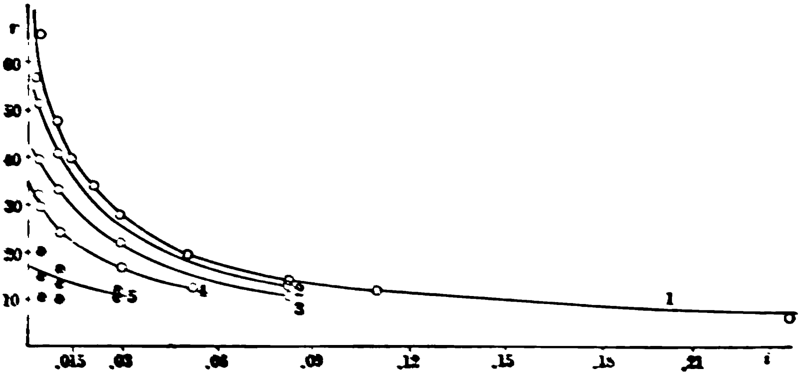


Fig 3

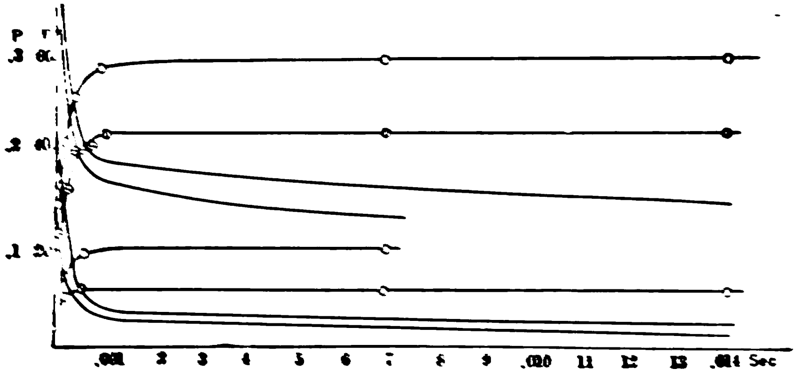


Fig. 4.

colored a dark green, but the uniformity of the readings indicate that the plates did not change throughout the experiment. The test of the resistance by the Kohlrausch method gave a value of thirty-three ohms, but the capacity effect was so large that the adjustment could not be made closer than about five per cent. The measurement of resistance by direct current indicated values much smaller than this. Later it appears that the direct current flowing only for a brief interval had an apparent temporary cleaning action upon the electrodes. The old plates were then thoroughly cleaned and immersed in the same vessel in another solution from the same stock bottle. The resistance now obtained by Kohlrausch's method was 3.9 ohms. The areas of each of these plates was 60 sq. cm. The primary circuit was closed approximately .08 of a second. Fig. 2 shows the relation of the polarizing electromotive force to the current. The ratio of these two quantities is the apparent resistance. In Fig. 3 resistances are plotted as ordinates and currents as abscissæ. The upper curve gives the apparent resistances. The other curves give resistances after allowing for the polarization measured after regular time intervals. The curve nearest the apparent resistance shows the resistance obtained when the polarization is measured, .00033 second after the removal of the polarizing electromotive force. This is the usual character of the curve obtained by the various experimenters, though the time period usually is several times longer than .00033 second. As this time interval is shortened, the resistances diminish and show less variation with changes of current. Some observations are shown on this curve for an unknown but very short time period, the data for which are not recorded. For more minute time period one could expect the curve for resistance to run parallel with the axis of abscissæ, *i. e.*, the resistance would be the same for all currents and probably the same as the electrolytic resistance. Fig. 4 gives the relation of polarization and of resistance to time for four different electromotive forces. As is apparent, the great changes all occur in the first one-thousandth of a second. There are also greater changes for small than for large electromotive forces.

Table III. gives some results obtained with small copper plates with one-tenth the area of the previous plates. The electrolytic resistance of the solution by the Kohlrausch method was 8.5 ohms when the plates were clean and 13.6 ± 0.4 when they had stood in the solution for some time. In this experiment the plates had stood in the solution twenty-four hours. The results are shown graphically in curves *a* and *b*, Fig. 5.

TABLE III.

$T \times 10^6$	$R_0 = 73.5$ $i = .00186$ $E = .1367$		$R_0 = 51.4$ $i = .00535$ $E = .3576$	
	P	r	P	r
12	.1098	13.36	.209	12.36
30	.1049	17.10	.200	14.02
60	.1004	19.51	.190	15.86
180	.0955	22.14	.1825	17.91

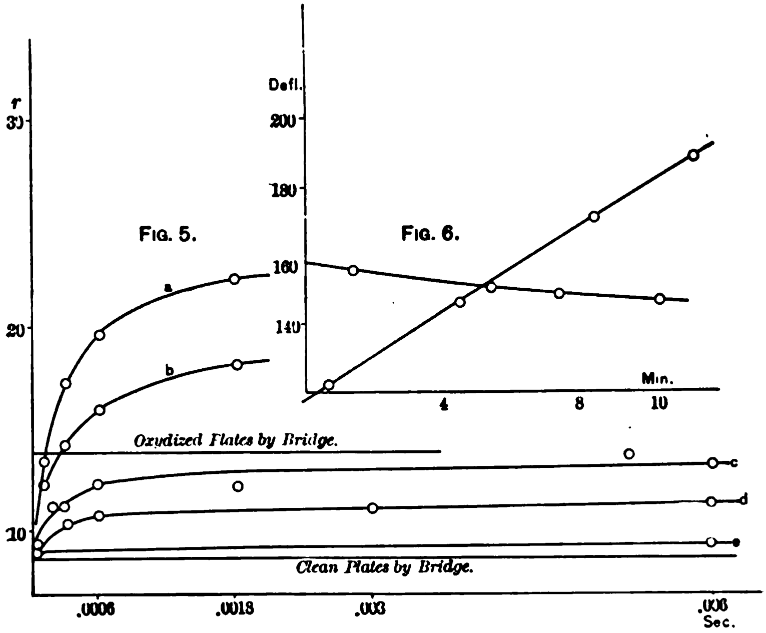


FIG. 5.

FIG. 6.

Tables IV. and V. give results for plates of the same size as in Table III. The data, however, are for plates when first placed in the solution. Oxidation occurs so rapidly that it was impossible at first to get any consistent results. The resistance increased continually as indicated both by the potential and the bridge methods. In twenty minutes the plates would be appreciably colored and had to be recleaned. The fresh prepared plates would give new values so different from the others that the difference could not be accounted for upon the ground of experimental error. Measurements were then taken at stated intervals and when these were exterpolated to zero time they became acceptably uniform. This indicated that the plates, if always cleaned with care and by the same method, would give almost the same resistance when first placed in the solution. The effects of oxidation increased the resistance fairly uniformly from this instant, but the effects were no two times alike and hence the above irregularities. It therefore became necessary to clean the plates for each determination. The circuits were then adjusted for the measurement of polarization, the plates dropped into the solution and about two seconds later the pendulum allowed to swing. The adjustments were then

Table IV.

R_v	i	E
16.11	.00050	.0081
15.2	.00193	.0283
13.12	.00454	.0596
12.55	.00587	.0737
12.6	.04580	.6771
11.	.08980	.9878

Table V.

	$R_v = 15.42$ $i = .00193$ $E = .0298$		$R_v = 12.71$ $i = .00587$ $E = .0745$		$R_v = 11.$ $i = .0898$ $E = .9878$	
	P	r	P	r	P	r
8	.0119	9.26	.0217	9.02		
18	.0083	11.11				8.8
30	.0083	11.11	.0147	10.21		
60	.0062	12.22	.0123	10.61		
180	.0064	12.1				
300			.0102	10.98		
600	.0054	13.15	.0087	11.22		9.2

made for drop over voltameter, *V*, or over resistance, *R* of 30 ohms in series with *V*, and after a definite time interval the pendulum was again dropped. An example of the observations for fall of potential over *V* and *R* is given in Table VI. and shown graphically in Fig. 6. The intercept of these curves on the *y* axis is taken for the true drop over *R* and *V*. The plates were then recleaned and the keys set for another period of polarization and observations repeated. Any slight variation in voltameter resistance gives a corresponding variation in applied potential, polarization and current. The mean of these values is recorded in Table IV. A more complete set of observations than given in Table V. is desirable, but, owing to necessity of cleaning the plates, to obtain such a set is an extremely tedious task. The data for resistance in Table V. are shown graphically in curves *c*, *d* and *e* of Fig. 5. Curves *a* and *b* arose from polarizing electromotive forces lying between the potentials causing the polarization *d* and *e* and that is where *a* and *b* should be expected to fall if the plates used in Table III. had been clean instead of being oxidized. We may therefore say that a large part of the effect in *a* and *b* arises from a change in the condition of the surface of the electrodes and consequent changes in adjacent solution and that the effect in *c*, *d* and *e* arises largely from the true concentration series.

TABLE VI.

T min.	Drop over 30 ohms.	Drop over *V*.
.75		122.2
1.5	27.6	147.
4.5		
5.5	270.	
7.5	268.4	
8.5		171.6
10.5	267.6	
11.5		189.

In the next experiment the polarizing electromotive force varied from .0003 second to .1637 second. The copper plates were the same as in the previous experiment. The resistance external to voltameter was constant. As the time of the applied electromotive force increased the polarization also increased and there was, therefore, an increase in the terminal electromotive force. This occasions an increase in the apparent resistance of the voltameter and a corresponding decrease in the current. These results are shown in Table VII. For each polarizing period the polarization was measured at different intervals. The results obtained are given in Table VIII. Fig. 7 shows the results graphically. T_v represents the time the voltameter was polarized and T_p the time elapsing between breaking the circuit and measuring of the polarization. The ordinates represent electromotive forces for both sets of curves. The curves with ordinates increasing with abscissæ each correspond to a constant T_p and variable T_v. For the remaining curves T_v is constant and T_p variable. The abscissæ are nine times larger for variable T_p than for variable T_v. The former show the decrease of polarization when the polarizing electromotive force is removed; the latter show the increase of polarization when the current is applied to the voltameter.

TABLE VII.

T	i	R_v	E
.0003	.001954	16.84	.0329
.0030	.001927	23.12	.0430
0240	.001914	38.10	.0729
0634	.001895	48.64	.0923
.1637	.001885	54.13	.1010

TABLE VIII.

T_p	$T_v = .0003$		$T_v = .003$		$T_v = .024$		$T_v = .0634$		$T_v = .1637$	
	P	r	P	r	P	r	P	r	P	r
.00012	.0077	12.90	.0181	13.71	.0367	18.94	.0529	20.44	.0725	15.66
.00030	.0067	13.24	.0152	15.21	.0302	22.32	.0453	24.46	.0611	21.71
.00060	.0058	13.89	.0123	16.72	.0274	23.81	.0425	25.96		
.00120	.0058	13.89	.0093	18.28	.0257	24.71	.0414	26.51	.0575	23.63
.00300	.0058	13.89	.0078	19.09	.0257	24.71	.0409	26.78	.0518	25.56
.00600	.0056	14.02			.0221	26.60	.0388	27.88	.0518	26.66
.01800	.0043	14.69	.0075	19.54	.0166	29.49	.0300	32.52	.0404	32.70

r by alternating current $= 13.6$.

Fig. 8 shows the value of r as ordinates and T_p as abscissæ. They show no regularity as to each other. With more accurate measurements they possibly would be found to intersect the y axis at the point corresponding to the electrolytic resistance.

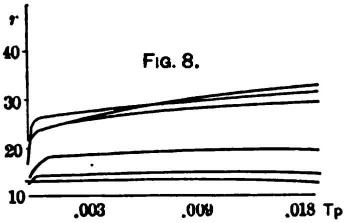


FIG. 8.

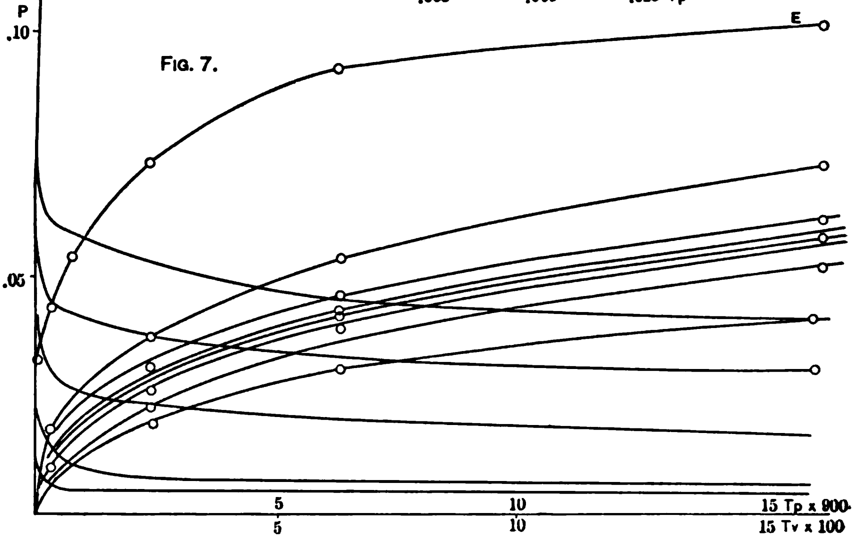


FIG. 7.

These observations show clearly the rapid formation of the polarization and that polarization arises for even minute current thrusts and for alternating currents. At first the current produces a change at the contact of solution and electrodes. It then effects deeper layers of the solution and the effect at the contacts of electrodes and solution increases, but less rapidly. This gives rise to further, but smaller increases in polarization. Diffusion diminishes the increase in polarization and after a time there is a constant potential between the terminals of the voltameter or equilibrium between the diffusion process and the increase in potential. On open circuit the diffusion still continues and affects most rapidly the strata nearest the electrodes were in many cases a reaction occurs. Therefore at the electrode surfaces the changes are greatest. When all the strata affected by the concentration changes are extremely thin, then the polarization will decrease very rapidly. This effect is shown in the polarization curves and the rapid changes of the apparent resistance when the applied current was very small.

When the currents become larger there are concentration changes farther from the electrodes, and the greater the distance from the electrodes to the outlying concentration changes the less P will diminish in a short interval. Therefore, the values obtained for the true resistance when large currents are used, provided they are not so large as to complicate the phenomena, correspond more nearly to values obtained with alternating currents. Since the circuits were not closed long enough to establish a maximum of polarization, one might anticipate that the same effect would be produced by increasing the length of time, the polarizing electromotive force was applied. In Table VIII. it is seen the polarization does increase as T_v increases and that the residual polarization is more persistent on open circuit for the larger values of T_v. However, when the current is applied for a longer time the curve for apparent resistance does not become more nearly horizontal but becomes more bent. This would indicate that as the polarizing electromotive force increases and the current decreases the part of the polarization at the contact of the electrode and the solution increases more rapidly than the part farther removed from the electrodes. Therefore, there is a greater change in polarization for about the same current, when the time for the polarizing electromotive force is increased. This point, however, needs further investigation. The intercept on the y axis of the E curve divided by the current gives the electrolytic resistance as measured by the alternating current. The polarization curves with varying T_v all pass through the origin. The value of the polarization T_v for $T_p = 0$ would be difficult to exterpolate. These polarization curves approach nearer the E curve as time increases and makes r apparently increase and later decrease with increasing time. This may be due to some inaccuracy in the measurements, but it is not altogether impossible since the polarization is very persistent when the time is longer as indicated by the curves where T_p is variable. The curves plotted with the last factor as variable indicate that there is a considerable part of the polarization very persistent. There is a part also which diminishes very rapidly, and which, by ordinary methods, has not been observed. The former probably concerns the strata of the concentration series more remote from the electrodes. The latter probably arises from the outer strata of the concentration series adjoining the electrode, where in the case of the oxidized plates reaction at once sets in. In case of clean copper plates this action would be scarcely noticeable, and therefore the slight variation of the *c*, *d* and *e* curves, Fig. 5. The curves and the data also indicate that if the true maximum value of P could be measured, the simple formula in ordinary use would give nearly the true resistance of the cell for all currents. That it should give the true electrolytic resistance when the measurements are very accurately measured should not be expected under all conditions. For concentration changes mean layers of different conductivity from the body of the solution. The heating of the electrodes may not be altogether negligible, particularly if either electrode is small and current density large. Gaseous products, and a change in the aggregation of the electrode surface also produce changes, since the current must either pass around or through them. Such phenomena as these together with the unmeasured part of the polarization, constitute what has been designated as a "transition resistance." It is always desirable to have as few complications as possible.

The time periods here used are so short one can estimate the polarization which would arise when an alternating current of slow frequency, like that used in electrolytic measurements, is applied. Let us assume a current alternating five hundred times to the second, having the same current strength as used in this experiment. Assume the average electromotive force to be seven-tenths of the maximum. Then the maximum would be reached in .001 second and the polarization would therefore be about .01 volt.

It is the author's intention to farther apply this method to thoroughly oxidized plates, and to clean electrodes, if oxidation of the latter can be prevented. Formaldehyde is recommended by some to prevent oxidation and will be tried. Farther tests for capacity; and for other solutions and other electrodes are desirable. To diminish the time period much more will not be possible since the shortest time already is near the limit of the time for the condenser to acquire full charge. However, a change in the apparatus so as to obtain greater accuracy in the measurement of the time is very desirable. The method is also applicable to measurement of internal resistance and polarization in primary batteries.

University of Nebraska, June, 1899.

A NEW METHOD OF COMPARING TWO SELF-INDUCTANCES.

HUBERT V. CARPENTER.

DURING a careful study of the more recent methods of measuring and comparing inductance and capacity I have developed the following method of comparing two self-inductances which gives very accurate results. Its freedom from the effects of other inductances permits its use for the comparison of very small values, and of values which are very different.

If l_1 and l_2 are the two coils whose coefficients of self-inductance, L_1 and L_2, are to be compared, connect the two in parallel having in series with one of them a variable non-inductive resistance. Apply to their common terminals an harmonic electromotive force. This will cause currents to flow through the coils which will lag behind the impressed electromotive force by the phase angles:

$$\varphi_1=\tan^{-1}\frac{\omega L_1}{R_1}, \text{ and } \varphi_2=\tan^{-1}\frac{\omega L_2}{R_2},$$

in which R_1 and R_2 are the total resistances of the two parallel branches including the coils l_1 and l_2, the variable resistance in one branch, and the connecting wires; while ω is equal to 2π times the frequency of the alternating current or $2\pi n$. This shows that when the two ratios $\frac{\omega L_1}{R_1}$ and $\frac{\omega L_2}{R_2}$ are equal the two currents in the parallel branch will be in the same phase. If, then, we can by any means produce and detect this condition of phase equality, we get at once the relation between the two inductances. It is simply, $\frac{\omega L_1}{R_1}=\frac{\omega L_2}{R_2}$, or $\frac{L_1}{L_2}=\frac{R_1}{R_2}$.

The two primary currents can be brought into phase equality by varying the non-inductive resistance in one of the branches. This condition can be detected in the following manner: Introduce a secondary circuit which is made up of two equal coils, s_1 and s_2, of many turns, and a telephone, all connected in series. Place coil s_1 close and parallel to l_1, coil s_2 close to l_2. The currents in l_1 and l_2 then induce electromotive forces in s_1 and s_2 respectively. The geometric sum of these two secondary electromotive forces will cause a current through the telephone. Suppose, however, the coils s_1 and s_2 to be so connected that when the currents in l_1 and l_2 are in phase with each other, the induced electromotive forces, Es_1 and Es_2 will be opposed to each other in the secondary circuit. The resultant secondary electromotive force, Es, will then be equal to $Es_1 - Es_2$; or, if these be made equal, will be zero. This condition will be detected by the silence of the telephone.

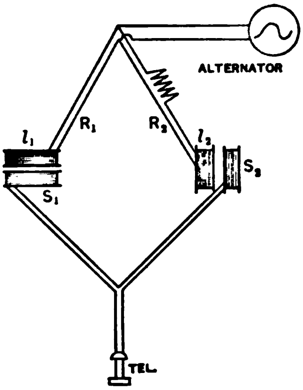


Fig. 1.

This silence indicates two things: 1st, that the two primary currents, Cl_1 and Cl_2, are in the same phase; 2d, that $Es_1 = Es_2$. Since the first condition is true, when $\frac{\omega L_1}{R_1} = \frac{\omega L_2}{R_2}$, it can be made true by varying R_1 or R_2. The second condition may be secured by varying the relative position of one of the secondary coils, S_2 for instance, with respect to its primary. This will change the electromotive force induced in it, and if it be moved back and forth until a position is found where the hum in the telephone is a minimum or zero we know that it is so placed that $Es_1 = Es_2$. If this adjustment does not give silence but only a minimum sound in the telephone it shows us that the two electromotive forces, Es_1 and Es_2, are not in exact opposition. This means that the two primary currents, Cl_1 and Cl_2, are not in the same phase and so R_1 or R_2 must be changed. After the value of R_1 or R_2, and the position of S_2 have been adjusted so as to give silence in the receiver, it is only necessary to know $\frac{R_1}{R_2}$ to get $\frac{L_1}{L_2}$.

The proper position of S_2 can be found so quickly that the work of obtaining a balance is but little more tedious than in the Wheatstone's bridge. When using the telephone as a current detector, however, there is no means of knowing which way R_1 or R_2 must be varied to reduce the phase difference between Cl_1 and Cl_2. This corresponds to the same weakness of the telephone when used in the Wheatstone's bridge.

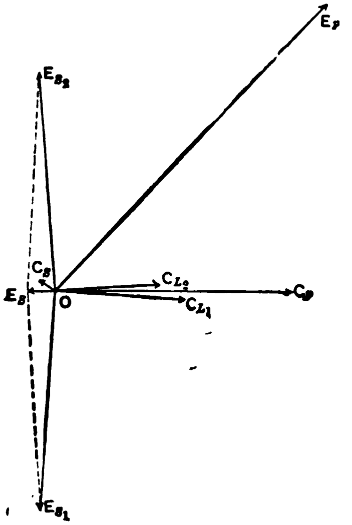


Fig. 2.

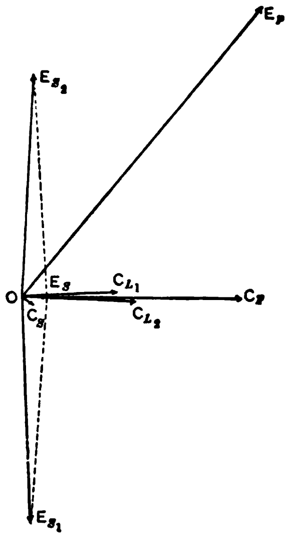


Fig. 3.

The phase relations of the currents and electromotive forces just discussed may be seen upon the two phase diagrams, Fig. 2 and Fig. 3. In these diagrams the vectors represent by their length the maximum values of the currents and electromotive forces, while their directions indicate their respective phases. The instantaneous values of all the quantities at any instant is given by the lengths of the projections of their respective vectors upon a moving vector which rotates in the clockwise direction through the entire 360° with each cycle of the current. According to this the secondary electromotive force, Es_1, reaches its maximum one quarter of a cycle later than the current Cl_1 which induces it, as it should. The vector Es_2 is shown reversed or 180° out of its true position. This corresponds to the physical condition of being connected in series with Es_1 in such a way that they oppose each other. As may be seen from the two diagrams, the resultant secondary electromotive force may have any phase whatever and varies with every change in the value of R_1 or R_2, or in the position of the coil S_2.

Careful tests were made of the method in this form. With a fairly good telephone, whose resistance was much too low for the greatest sensitiveness, successive comparisons could be made to agree within one-half of 1 per cent. without difficulty.

In order that these results may not only agree among themselves, but may be correct, the apparatus must be so arranged that there shall be no appreciable inductive effects except those which are accounted for. To do this the two coils under comparison, l_1 and l_2, which carry fairly large currents, must be so placed as to prevent mutual inductance between them. This was accomplished by placing them about two meters apart and in mutually perpendicular planes. Tests were made which showed this to be effectual. The lead wires from the source of the primary current to the point where the circuit divided were made up of twisted lamp cord and were kept as far as possible from the coils. The leads to each coil from this dividing point were placed about one centimeter apart and crossed over at frequent intervals. In comparing coils of low inductance it would be best to use lamp cord here also, as the effect of capacity would be very small compared with the inductive effect of the large current.

Any current which flows in the secondary circuit will, of course, react upon the primary currents, but when a balance is reached the secondary current disappears, and with it its disturbing influence. The secondary electromotive forces remain, however, and, together with the distributed capacity in the secondary coils, will set up a small local current in each coil. The amount of this hidden current is hard to estimate. It depends upon the capacity of the coil, which we can do no more than estimate, and its value is different at every point along the conductor. It can easily be shown to be very small however. Two pairs of coils were used at different times as secondaries, one pair of 300 turns each of No. 25 B. & S. wire and about 30 cm. in diameter; the other pair of 3000 turns each of No. 30 wire and about 10 cm. in diameter. The latter were made up of 50 layers of 60 turns each. If we were to consider each layer as a plate of a condenser with alternate plates connected together the calculated capacity would be about .012 microfarad. In the coil, however, the difference in potential between layers is only about one-fiftieth of the potential across the terminals of the coil so the effective capacity will be something like .0002 microfarad. Suppose the total electromotive force induced in the coil to be as great as 50 volts or one volt per layer, and assume that the resistance and self-induction of the coil does not prevent it from becoming fully charged at each impulse of the current. Then, if the current in the primary passes through sixty cycles per second the local current in each secondary coil will be :

$I = \frac{Q}{t} = \frac{EC}{t} = 120 \times 1. \times .0000000002$ or about $24. \times 10^{-9}$ ampères. If the coils of 300 turns each were used the local current would be much less than this. If this estimate be correct it shows that the reactive effect of these local currents upon the primary coils is entirely negligible. Further, it is easily seen that both primary coils are affected in the same way so that if the primaries were equal the error would be zero. Tests were made with both pairs of secondary coils upon the same primaries and the fact that no difference in the results due to the change if secondaries could be detected gives experimental evidence that the effect of the local currents is not measurable.

The next step in the development of the method was to replace the telephone by the electro-dynamometer. This required considerable change in details, but the principle remains the same, and the results showed a marked improvement in accuracy. The ordinary dynamometer having both coils connected in series is not sensitive to small currents, so a special instrument was used, in which the fixed coil was made up of 600 turns of No. 25 wire, and the suspended coil of 380 turns of No. 36. The latter was suspended by a phosphor bronze strip of about three circular mils cross-section and ten inches in length. This suspension strip and a similar strip in a loose spiral below the coil formed the connecting wires to the suspended coil. These strips would carry about .01 ampère for a short time, while the fixed coils of the instrument would safely carry 1,000 times as great a current for the same length of time. No metal was used in the instrument near the coils except the conductors themselves, because it was designed for use in testing Professor Rowland's phase angle methods. In the new method it is used only to detect small currents, and for this could be improved by adding more turns to the suspended coil and an iron core to the fixed coils.

The small secondary current, which is to be made zero, is passed through the suspended coil, and the heavy primary current through the fixed coils of the dynamometer. In this way the instrument was made sensitive to currents of about $10.^{-8}$ ampères. The directian of the deflection of the suspended coil reverses when the phase of the current in either coil is changed by 180°, so the instrument will give an indication concerning which way the resistances of the primary branches must be changed to bring the balance nearer. This is another advantage over the telephone. One peculiarity of the dynamometer must be remembered, however. If the two currents in the two coils differ by 90° in phase there will be no deflection. It becomes necessary, then, to distinguish between a zero dedeflection due to zero secondary current and a zero deflection due to 90° phase difference between the primary and secondary currents. The phase relations between these two currents may be seen in the phase diagrams, Figs. 2 and 3. We see that without some other arrangement it is impossible to tell whether the dynamometer is indicating zero secondary current or 90° phase difference. This difficulty was met in the following way: A non-inductive shunt, R_n, was connected across the terminals of the fixed coils of the dynamometer, with a key in its circuit so that it could be cut out at will. When this is connected the main primary current is divided between the two paths, and since the coils have considerable inductance while the shunt has not, the current in the fixed coils will lag behind the total primary current by a considerable angle. The currents and electromotive forces of the coils, l_1, l_2, s_1, s_2, do not change their relative phases in any way when this shunt connection is made, so the result is that the difference in phase between the two dynamometer currents is altered. If it was 90° before the shunt was connected, it must be something more or less afterwards, and unless the secondary current is zero, there will be a deflection of the suspended coil. This result could have been attained easily in other ways, but this seemed the simplest and proved very satisfactory.

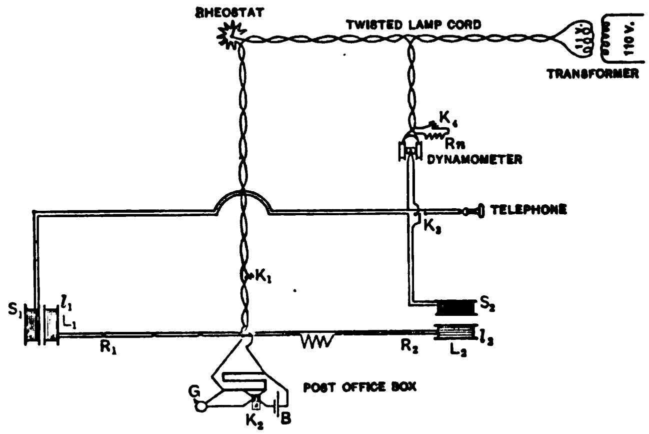


Fig. 4.

The connections as finally used are shown in the large diagram, Fig. 4.

The theory so far given is dependent upon the assumption that the electromotive force impressed upon the primary circuit is an harmonic function of the time. Since, however, the ratio of inductance to resistance in the two branches of the primary circuit are made the same at the final balance, it is true that, no matter what electromotive force is applied to their common terminals the currents in the two branches will rise and fall together. The two secondary electromotive forces will then have the same wave form and phase, and can be made to neutralize each other completely. This was shown to be true experimentally by the fact that when a balance had been obtained by the use of the dynamometer and an alternating current, no sound would be heard in the telephone when an interrupted current was used. This extreme test is certainly ample verification. The question of the source of the electromotive force is now an easy one, for any alternating current of any frequency, wave form, or degree of unsteadiness may be used. The tests given herewith were made with current from a two-phase, 60-cycle, Westinghouse generator. No tests of the wave form were made but the makers state that it differs from a sine curve by less than five per cent. at every point.

Let us consider the conditions under which the method will give the best results. In short, what is the relation between L_1 and R_1, or between L_2 and R_2, for maximum sensitiveness? The resultant electromotive force in the secondary circuit, E_s, also the secondary current, C_s, upon which the observations depend, varies, for small differences, with the angle of phase difference between Cl_1 and Cl_2. For maximum sensitiveness a given small change in the value of R_1 or R_2 should produce a maximum change in this angle, θ, of phase difference; or, the rate of change of R_1 with respect to θ should be a minimum. θ is equal to $\varphi_1 - \varphi_2$, where φ_1 and φ_2 are the phase angles by which the currents Cl_1 and Cl_2 lag behind the primary electromotive force, and to produce a change in θ we change either φ_1 or φ_2 while the other is unaltered. It follows then that a given change in φ_1 will produce an equal change in θ. This being true we can say that, for maximum sensitiveness, the rate of change of R_1 with respect to φ_1 must be a minimum.

The relation between R_1 and φ_1 is,

$$\tan \varphi_1 = \frac{\omega L_1}{R_1}$$

or,

$$R_1 = \omega L_1 \cot \varphi_1$$

The rate of change of R_1 with respect to φ_1 is then:

$$\frac{dR_1}{d\varphi_1} = - \omega L_1 csc^2 \varphi_1.$$

This rate will be a minimum when:

$$\frac{d^2 R_1}{d\varphi_1^2} = 0, \text{ or when } 2\omega L \cot \varphi_1 csc^2 \varphi_1 = 0.$$

Substituting for the trigonometric functions their values in terms of ωL_1 and R_1 we have,

$$2 \cdot \omega L_1 \cdot \frac{R_1}{\omega L_1} \cdot \frac{R_1^2 + \omega^2 L_1^2}{\omega^2 L_1^2} = 0.$$

Since R_1 is the only variable included in the expression we see that the most accurate results will be attainable when the resistances R_1 and R_2 are as near zero as possible. In other words, the greater the angle by which the primary currents lag behind the electromotive force the greater will be the accuracy of the results. The formulæ also show that the sensitiveness when the primary currents lag 45° is one-half of the theoretical maximum. This is attainable in the case of most ordinary coils with a frequency of sixty.

As may be seen from Fig. 4 the telephone and dynamometer were connected so that either could be used in the secondary circuit. In practice the telephone was used in the preliminary adjustments and the dynamometer for obtaining the balance. The post-office box was connected to the central posts where the leads to the primary coils were joined and as soon as an adjustment was completed a lead wire of one coil, say l_1, was disconnected and the resistance of l_2 measured; then l_2 was disconnected and l_1 reconnected and its resistance measured. In this way R_1 and R_2 were determined before any appreciable change could occur for care was always taken to avoid heating the coils. The postoffice box used was the best in the laboratory and can be depended upon to give results within $\frac{1}{10}$ % of the correct values while the ratios given by it are considerably more accurate because the two resistances were measured under the same conditions and as far as possible the same coils in the box were used. The resistances given below have all been corrected for the resistance of the leads from the postoffice box to the common terminals of the coils.

The inductances of five different coils were compared. The coils will be designated, l_1, l_2, l_3, l_4, and l_5, with inductances, L_1, L_2, L_3, L_4 and L_5, respectively. Coils l_1 and l_2 were wound in regular layers in a rectangular groove cut in a disk of plaster of paris, and their dimensions accurately determined so that their inductances could be calculated. Coil l_1 was made up of 70 turns in 7 layers of No. 20, B. & S. gauge, single silk-covered copper wire. Its mean diameter was 38.605 cm. Calculated inductance, $L_1 = .0049046$ henry.

Coil l_2 was made up of 8 turns in 2 layers of No. 15 B. & S gauge, double cotton-covered copper wire. Its mean diameter was 38.681 cm. Calculated inductance, $L_2 = .0000726$ henry. These inductances were calculated by the formula due to Maxwell and Stefan.[1] The value calculated for L_1 was assumed to be correct and made the basis for finding the values of the others from the ratios. Coils l_3, l_4 and l_5 were of unknown dimensions and wound on wooden frames.

---

[1] Stefan, Wied. Ann., V. 22, p. 107, Apr., 1884.

DATA. TABLE I.

COMPARISON OF COILS l_1 AND l_3.

R_1	R_3	L_3	
3.1574	27.865	.043285	
3.1198	27.559	.043324	
3.1104	27.479	.043327	
3.0940	27.363	.043375	
3.0690	27.115	.043330	
		Average =	.043328
	Probable error =	.000010 =	.02 + per cent.

TABLE II.

COMPARISON OF COILS l_1 AND l_4.

R	R_4	L_4	
2.9785	30.828	.050762	
2.9900	30.985	.050825	
3.0212	31.209	.050662	
3.0380	31.459	.050790	
2.9911	30.978	.050793	
2.9068	30.038	.050682	
2.9542	30.594	.050790	
2.9357	30.427	.050830	
		Average =	.050767
	Probable error =	.000014 =	.03 — per cent.

TABLE III.

COMPARISON OF COILS l_3 AND l_4.

R_3	R_4	L_4	
26.855	31.450	.050741	$L_4 = \frac{R_4}{R_3} L_3$ where $L_3 = .043328$
26.905	31.547	.050802	
26.897	31.540	.050808	
26.873	31.427	.050665	
26.800	31.406	.050774	
27.023	31.647	.050740	
26.915	31.513	.050730	
26.950	31.520	.050675	
		Average =	.0507445
	Probable error =	.000012 =	.02 + per cent.

TABLE IV.

COMPARISON OF COILS l_1 AND l_5.

R_1	R_5	L_5	
3.1641	.26870	.00041650	
3.1800	.26911	.00041510	
3.1905	.27033	.00041555	
3.1847	.27009	.00041593	
3.1838	.26996	.00041585	
3.1775	.27013	.00041695	
3.2108	.27266	.00041650	
		Average =	.00041605
	Probable error =	.00000016 =	.04 — per cent.

TABLE V.

COMPARISON OF COILS l_2 AND l_5.

R_5	R_2	L_2	
.76554	.13177	.000071615	
.76513	.13182	.000071680	
.76579	.13185	.000071628	
		Average =	.000071641

The values given for L_2 in Table V. are based on the value of L_5 found in Table IV. This depends upon the calculated value of L_1, so that in the approximate agreement between the above value and the calculated value of L_2 we have an inaccurate check upon the correctness of both. Table III. gives a direct check upon the correctness of the ratios $\frac{L_1}{L_3}$, $\frac{L_1}{L_4}$ and $\frac{L_3}{L_4}$. From the data there given the ratio of L_3 to L_4 is obtained, and the values of L_4 given are based upon the value of L_3 obtained in Table I. The two values L_4 differ by 0.05%, but this represents the combined error in three independent sets of observations.

The method here outlined might also be applied to the comparison of two capacities by connecting one in series with each primary coil l_1 and l_2. The results, however, would require correction for the inductances L_1 and L_2, and, further, unless the two condensers were exactly similar in construction, the result would be in error, because of the unequal rates of absorption.

PHYSICAL LABORATORY, UNIVERSITY OF ILLINOIS, April, 1899.

NEW BOOKS.

Lehrbuch der Experimentalphysik. Vierter Band; *Die Lehre von der Strahlung.* By ADOLPH WÜLLNER. Leipzig: B. J. Teubner, 1899.

The fourth volume of the fifth edition of this standard treatise deals with radiation. Its new title differs from that of the corresponding volume of the earlier editions, which was entitled *Die Lehre vom Lichte.* The work has grown in the attempt to incorporate the results of the most important newer investigations from 624 pages in the third edition of 1875, to 1,042 pages in the edition of 1899.

This volume shows indeed a greater amount of new material than any of the other volumes of this edition, and while the plan of the work remains unchanged, one is pleased to recognize the introduction of full and very satisfactory descriptions of such investigations as that of Wiener, on "Standing Light Waves," of Kerr on "Electro-optic Double Refraction," of Drude and of Rubens on "Metallic Refraction," etc.

The most important portions of Zeemann's work on the influence of "Magnetization and the Emission of Light," appeared too late to find a place in this edition, but his earlier papers of 1897 are briefly described.

E. L. N.

Ostwald's *Klassiker der Exakten Wissenschaften.* No. 99, *Ueber die Bewegende Kraft der Wärme,* by R. CLAUSIUS. No. 100, *Abhandlungen über Emission und Absorption,* by G. KIRCHHOFF. Edited by Professor MAX PLANCK. Leipzig: Wilhelm Englemann, 1898.

With these two volumes, this well-known series of German scientific-reprints reaches its 100th number. Of the two memoirs selected by the editor, Professor Planck, for reprinting, the papers of Kirchhoff were perhaps more in need of being brought into convenient and accessible form than the well-known work of Clausius, on the "Motive Power of Heat." Both indeed are easily accessible in German, the latter having been reproduced in the first edition of Clausius's treatise while Kirch' hoff's memoirs are to be found not only in the Transactions of the Berlin Academy of Science, a set which is not always at one's disposal, but likewise in his volume of *Gesammelten Abhandlungen.* Of Clausius paper there exist at least three literal English versions. The value to science in these reprints will be found, therefore, rather in the two brief notes of the editor than in the reproduction of the texts themselves.

E. L. N.

Inorganic Chemical Preparations. By FELIX LENGFELD. Pp. xvii+55. New York, The Macmillan Co., 1899. (*Received.*)

Laboratory Manual. Experiments to illustrate the Elementary Principles of Chemistry. By H. W. HILLYER. Pp. vi+200. New York, The Macmillan Co., 1899. (*Received.*)

Standard Text-Books on General Physics

Elements of Theoretical Physics

By **DR. C. CHRISTIANSEN**
Professor of Physics, University of Copenhagen
TRANSLATED INTO ENGLISH BY
W. F. MAGIE, Ph.D., Professor of Physics, Princeton University
Cloth. 8vo. $3.25, net

Problems and Questions in Physics

CHARLES P. MATTHEWS, M.E., AND **JOHN SHEARER, B.S.**
Purdue University *Cornell University*
8vo. Cloth. pp. 247 + 4. Price $1.60, net

An Intermediate Course of Practical Physics

By **A. SCHUSTER, Ph.D., F.R.S.,** AND **C. H. LEES, D.Sc.**
Owens College, Manchester *Owens College, Manchester*
12mo. Cloth. pp. xv + 248. Price $1.10, net

Lessons on Elementary Practical Physics

By **BALFOUR STEWART, A.M., LL.D., F.R.S.,** AND **W. W. HALDANE GEE**

Vol. I.		**General Physical Processes.** 12mo.	$1.50, *net*
Vol. II.		**Electricity and Magnetism.**	$2.25, *net*
Vol. III.	Part I.	**Practical Acoustics.**	$1.10, *net*
	Part II.	**Heat and Light.**	*In Press*

Laboratory Manual of Physics and Applied Electricity

Arranged and Edited by **EDWARD L. NICHOLS**
Professor of Physics in Cornell University
IN TWO VOLUMES
Vol. I. Cloth. Price $3.00, net

JUNIOR COURSE IN GENERAL PHYSICS. By ERNEST MERRITT and FREDERICK J. ROGERS.

Vol. II. Cloth. pp. 444. Price $3.25, net

SENIOR COURSES AND OUTLINES OF ADVANCED WORK By GEORGE S. MOLER, FREDERICK BEDELL, HOMER S. HOTCHKISS, CHARLES P. MATTHEWS, and THE EDITOR.

A large proportion of the students for whom primarily this Manual is intended are preparing to become engineers, and special attention has been devoted to the needs of that class of readers. In Vol. II., specially, a considerable amount of work in applied electricity, in photometry, and in heat has been introduced.

THE MACMILLAN COMPANY
New York: 66 Fifth Avenue
BOSTON CHICAGO SAN FRANCISCO

STANDARD BOOKS ON
ELECTRICITY AND MAGNETISM

The Theory of Electricity and Magnetism

By **A. G. WEBSTER, A.B. (Harv.), Ph.D. (Berol.)**

Assistant Professor of Physics, Director of the Physical Laboratory, Clark University, Worcester, Massachusetts

8vo. Cloth. pp. xii + 576. Price, $3.50, net

Elementary Lessons in Electricity and Magnetism

By **SILVANUS P. THOMPSON, D.Sc., B.A., F.R.A.S.**

New Edition. With Illustrations. 16mo. Cloth. $1.40, net.

"I consider Thompson the best book of its size and nature on the subject of electricity, and have had frequent occasion to recommend it to friends and students. It is the only book that is not simply a earrangement or reproduction of the old treatise."

—WILLIAM A. HALLOCK, Ph.D., *Columbia University, New York City.*

The Principles of the Transformer

By **FREDERICK BEDELL, Ph.D.**

Assistant Professor of Physics in Cornell University

8vo. Cloth. 250 Illustrations. Price, $3.25, net

This work constitutes a systematic treatise on the alternating Current Transformer, and a logical exposition of the principles involved, of the most modern methods of transformer design, construction, and testing, and of transformer systems of distribution.

A Text-Book on Electro-Magnetism

AND THE CONSTRUCTION OF DYNAMOS

By **DUGALD C. JACKSON, B.S., C.E.**

Professor of Electrical Engineering, University of Wisconsin

12mo. Cloth. Price, $2.25, net

Alternating Currents and Alternating Current Machinery

By **DUGALD C. JACKSON, C.E.** and **JOHN P. JACKSON, M.E.**

University of Wisconsin *Pennsylvania State College*

12mo. Cloth. pp. xvii + 729. Price, $3.50, net

"I like the way in which they develop their theory and make practical applications. I feel sure that the book will receive a hearty welcome."

—GEO. W. PATTERSON, Jr., *Asst. Professor of Physics, University of Michigan*

The Alternating Current Circuit

By **W. P. MAYCOCK, M.I.E.E.**

An Introductory and Non-Mathematical Book for Engineers and Students. With 51 Illustrations, Index, and Ruled Pages for Notes.

16mo. Cloth. pp. viii + 102. Price, $1.00

The Storage Battery

A PRACTICAL TREATISE ON THE CONSTRUCTION, THEORY AND USE OF SECONDARY BATTERIES,

By **AUGUSTUS TREADWELL, Jr.**

Cloth. 12mo. pp. xx + 257. Price, $1.75, net

JUST READY

Electricity in Town and Country Houses

By **PERCY E. SCRUTTON.**

Second Edition. Fully Illustrated. 12mo. Cloth. pp. xii + 148. Price, $1.00.

A brief and simple statement of the first principles of the application of electricity to the needs of the home, for heating, cooking, lightning, etc. A description is given of the plants required for towns of various size, and there are numerous illustrations showing examples of electric appliances. It is a popular and untechnical book, and intended for those who have not the time necessary for a study of more strictly scientific works.

THE MACMILLAN COMPANY

New York: 66 Fifth Avenue

BOSTON **CHICAGO** **SAN FRANCISCO**

Lectures on Elementary Mathematics

By JOSEPH LOUIS LAGRANGE

Being the Course of Lectures Delivered at the École Normale, Paris, 1795

Translated from the French by THOMAS J. McCORMACK.

With a fine photogravure Portrait of the Great Mathematician, Notes, Biographical Sketch of Lagrange, Marginal Analyses, Index, etc. Handsomely Bound in Red Cloth. Pages 172. Price, $1.00 net (5s.).

A Masterpiece of Mathematical Exposition

First Separate Edition in English or French

"I intend to recommend Lagrange's *Lectures on Elementary Mathematics* to the students of my course in the Theory of Equations, for collateral reading, and also to the teachers of elementary mathematics who attend my summer conferences on the Pedagogy of Mathematics. I trust that this valuable translation is but the forerunner of others in the same field by your house."—*J. W. A. Young*, Professor of Mathematics in the University of Chicago.

"I put the book aside for a careful and leisurely perusal, in the first rush of my return from Japan and now have reached it, and am truly surprised at it. It makes Lagrange human and personal to me How he knew his Euclid!"—*Dr. George Bruce Halsted*, Prest. Texas Academy of Science.

"Probably never equalled in the peculiar quality of leading the mind to see and enjoy the beauty as well as the accuracy of the science. It sets forth not only the bare science, but makes clear its correlation with history and philosophy."—*The Chicago Chronicle*.

"The book ought to be in the hands of every high-school teacher of mathematics in America, for the sake of getting Lagrange's point of view."—*Prof. Henry Crew*, Northwestern University Evanston, Ill.

THE OPEN COURT PUBLISHING CO., CHICAGO, 324 Dearborn St.

LONDON: Kegan Paul, Trench, Trübner & Co.

ON THE STUDY AND DIFFICULTIES OF MATHEMATICS

By AUGUSTUS DE MORGAN

Just Published—New corrected and annotated edition, with references to date, of the work published in 1831 by the Society for the Diffusion of Useful Knowledge. The original is now scarce.

With a fine Portrait of the great Mathematical Teacher, Complete Index, and Bibliographies of Modern Works on Algebra, the Philosophy of Mathematics, Pangeometry, etc.

Pp. viii+288. Cloth, $1.25 (5s.).

"**A Valuable Essay.**"—PROF. JEVONS, in the *Encyclopædia Britannica*.

"The mathematical writings of De Morgan can be commended unreservedly."—PROF. W. W. BEMAN, University of Michigan.

"It is a pleasure to see such a noble work appear as such a gem of the book-maker's art."—PRINCIPAL DAVID EUGENE SMITH, Brockport Normal School, N. Y.

THE OPEN COURT PUBLISHING CO., CHICAGO, 324 Dearborn St.

LONDON: Kegan Paul, Trench, Trübner & Co.

The Astrophysical Journal

AN INTERNATIONAL REVIEW OF SPECTROSCOPY AND ASTRONOMICAL PHYSICS

EDITED BY

GEORGE E. HALE
Director of the Yerkes Observatory

AND

JAMES E. KEELER
Director of the Lick Observatory

WITH THE COÖPERATION OF

THE LEADING ASTROPHYSICISTS OF THE UNITED STATES AND EUROPE

Contents of Recent Numbers

FEBRUARY 1898

On the Arc-Spectra of the Elements of the Platinum Group. I. . . . . H. KAYSER

The Aberration of Parabolic Mirrors
CHARLES LANE POOR

On the Causes of the Sun-Spot Period
E. J. WILCZYNSKI

Radiation in a Magnetic Field
A. A. MICHELSON

Minor Contributions and Notes

Reviews

MARCH 1898

The Concave Grating for Stellar Photography
CHARLES LANE POOR and S. ALFRED MITCHELL

On Certain New Results relating to the Phenomena Discovered by Dr. Zeeman
A. CORNU

Resumé of Solar Observations made at the Royal Observatory of the Roman College during the Second Half of 1897
P. TACCHINI

On the Arc-Spectra of the Elements of the Platinum Group. II. . . . . H. KAYSER

Minor Contributions and Notes

Reviews

Recent Publications

APRIL 1898

On the Spectrum of Hydrogen in the Nebulae . . . . J. SCHEINER

On the Level of Sun-Spots and the Cause of their Darkness . . . . A. L. CORTIE, S. J.

Sources of Error in Investigations on the Motion of Stars in the Line of Sight
H. C. VOGEL

The Variation of Solar Radiation
FRANK W. VERY

Arc-Spectrum of Vanadium
HENRY A. ROWLAND and CALEB N. HARRISON

Minor Contributions and Notes

Reviews

MAY 1898

Theoretical Considerations respecting the Dependence of Wave-Lengths on Pressure which Messrs. Humphreys and Mohler have observed in the Arc-Spectra of Certain Elements . . . . J. WILSING

Correspondence of the Photographic Durchmusterung with the Visual . R. H. TUCKER

The Caustic of the Right Parabolic Cylinder
C. W. CROCKETT

The Parabolic Mirror . . . . C. W. CROCKETT

Comparison of Oxygen with the extra Lines in the Spectra of the Helium Stars, B Crucis, etc.; also Summary of the Spectra of Southern Stars to the 3½ Magnitude and their Distribution . . . . FRANK McCLEAN

Arc-Spectrum of Zirconium and Lanthanum
HENRY A. ROWLAND and CALEB N. HARRISON

Minor Contributions and Notes

CHICAGO
The University of Chicago Press
LONDON: WM. WESLEY & SON

Published every month except July and September

Subscription Price
Domestic, $4.00; Foreign, $4.50

PORTRAITS

To satisfy a demand which has arisen, the publishers have arranged to issue a limited number of copies of the portraits of scientific men, that have appeared from time to time as frontispieces in the PHYSICAL REVIEW.

The portraits are printed in photogravure, on plate paper, suitable for framing, and are for sale at the very low price of 25 cents each.

NOW READY.

JOHN TYNDALL.
HEINRICH HERTZ.
HERMAN VON HELMHOLTZ (1876).
AUGUST KUNDT.
ALFRED M. MAYER.
WILLIAM A. ROGERS.

THE PHYSICAL REVIEW

A Journal of Experimental and Theoretical Physics

CONDUCTED BY

EDWARD L. NICHOLS, ERNEST MERRITT and FREDERICK BEDELL

XLVIII. October, 1899

TABLE OF CONTENTS

On the Magnetic Shielding Effect of Tri-lamellar Spherical and Cylindrical Shells. A. P. WILLS.

A New Respiration Calorimeter and Experiments on the Conservation of Energy in the Human Body, II. W. O. ATWATER and E. B. ROSA.

New Books: *Hyndman:* Radiation, An Elementary Treatise on Electromagnetic Radiation and on Röntgen and Cathode Rays.

Notes: American Association for the Advancement of Science; International Congress of Physicists.

XLIX. November-December, 1899

TABLE OF CONTENTS

Frontispiece

On the Freezing Points of Aqueous Solutions of Non-electrolytes. E. H. LOOMIS.

An Absolute Determination of the E. M. F. of the Clark Cell. HENRY S. CARHART and KARL E. GUTHE.

The Resistance Offered by Iron Wires to Alternating Currents. ERNEST MERRITT.

On a Theorem of Clebsch's. PAUL SAUREL.

An Investigation of the Magnetic Qualities of Building Brick. O. A. GAGE and H. E. LAWRENCE.

Note: Robert Wilhelm Bunsen

New Books: *Hastings and Beach:* A Text-Book of General Physics; *William Watson:* Practical Physics; *Harper's Scientific Memoirs:* The Laws of Gases; The Second Law of Thermodynamics; The Fundamental Laws of Electrolytic Conduction

Index

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

VOLUME X NUMBER 2

THE
PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, and FREDERICK BEDELL

LI

FEBRUARY 1900

TABLE OF CONTENTS

The Influence of Electrification on the Surface Tension of Water and Mercury 65
ERNEST MERRITT and SAMUEL J. BARNETT

An Objective Representation of the Hysteresis of Iron and Steel 74
KNUT ÅNGSTRÖM

A Determination of the Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether. II **REGINALD A. FESSENDEN** 83

Thermodynamic Notes (No. 10); Revision of Some Thermal Data Concerning Benzene **K. TSURUTA** 116

An Efficient Boyle's-Law Apparatus **W. J. HUMPHREYS** 123

New Books: *Scheiner:* Strahlung und Temperatur der Sonne; *Gerland* und *Traumüller:* Geschichte der Physikalischen Experimentierkunst. 126

COPYRIGHT, 1899, BY THE MACMILLAN COMPANY

PUBLISHED FOR CORNELL UNIVERSITY
BY THE MACMILLAN COMPANY
LANCASTER, PA., AND NEW YORK
LONDON: MACMILLAN AND CO., LTD
BERLIN: MAYER AND MUELLER

THE PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, AND FREDERICK BEDELL

Two volumes of THE PHYSICAL REVIEW are published annually, these volumes beginning in July and January, respectively, and containing at least five numbers each. The price of subscription is two dollars and fifty cents a volume (five dollars a year), or fifty cents a number. Subscriptions should be sent to the publishers. THE MACMILLAN COMPANY, 41 *North Queen Street, Lancaster, Pa.*, or 66 *Fifth Avenue, New York;* Messrs. MACMILLAN & CO., LTD., *London;* or to Messrs. MAYER AND MUELLER, *Berlin.*

Previous to Volume V. (July–December, 1897) THE PHYSICAL REVIEW was published in annual volumes, each containing six bimonthly numbers, beginning with the July–August number, 1893. These may be obtained from the publishers at the former subscription price, three dollars per volume.

Subscribers who desire to have their copies of the REVIEW sent to them with the edges cut, can so obtain them by informing the publishers to that effect.

Correspondence relating to contributions should be addressed to the editors, at *Ithaca, New York.*

Manuscript intended for publication in THE PHYSICAL REVIEW must be communicated by the author; when publication in other journals is contemplated, notice to that effect should be given.

The authors of original articles published in the REVIEW will receive one hundred separate copies in covers, for which no charge will be made; additional copies, when ordered in advance may be obtained at cost.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

The

General Electric Company's

Pocket Ammeters

INCLINED COIL POCKET AMMETER (Covered)

of twenty-five amperes' capacity can be used upon either direct or alternating current, and gives accurate readings for experimental work in the laboratory.

INCLINED COIL POCKET AMMETER (Uncovered)

This pocket ammeter is one of a complete set of portable inclined coil electrical measuring instruments, for use in the laboratory, central station and outside work.

GENERAL ELECTRIC COMPANY

Main Office: SCHENECTADY, N. Y.

Sales Offices in All Large Cities

For CANADA: Address the CANADIAN GENERAL ELECTRIC COMPANY, Toronto, Can.

WM. GAERTNER & CO.

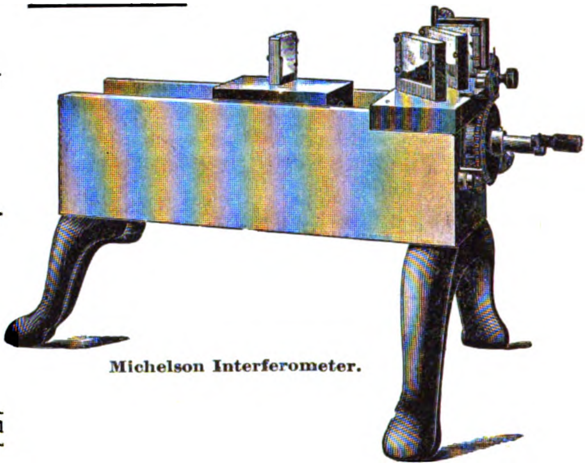
Astronomical & Physical Apparatus

152-158 E. 56th Street, CHICAGO

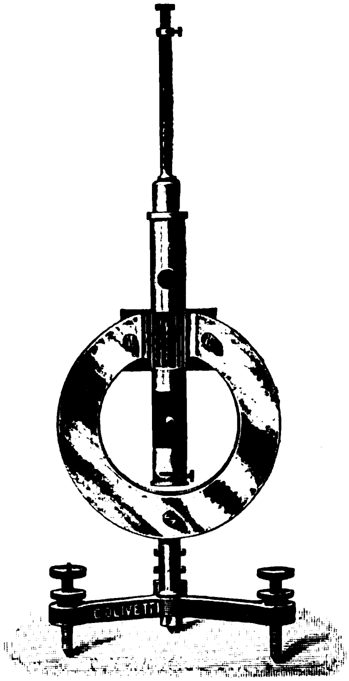
SPECIALTIES

- Standard Apparatus of new and improved designs
- Astronomical Telescopes
- Spectroscopes
- Micrometers
- Michelson Interferometers
- Reading-microscopes and Telescopes
- Bolometers
- Heliostats
- Dividing Engines, Comparators
- General Laboratory Apparatus
- Universal Laboratory-supports

Special attention paid to the construction of instruments for original investigation after our own or furnished designs.



Michelson Interferometer.



Professors and Assistants in electrical laboratories don't like to give students fine galvanometers because such instruments are liable to be spoiled.

They are perfectly right

Students don't like to work with cheaply made instruments, because they are not correct and are troublesome to handle.

They are perfectly right

Use Olivetti's Reflecting Galvanometers and you will have high-grade up-to-date instruments which don't fear hard service.

Catalogues and prices for the asking

CAMILLO OLIVETTI

Maker of Electrical Instruments

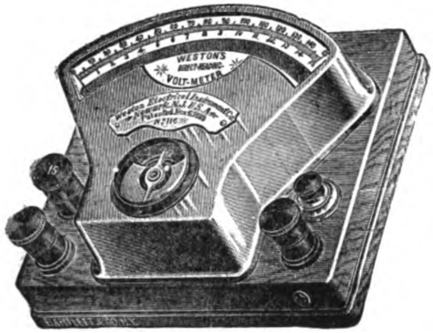
IVREA, ITALY

WESTON

STANDARD
PORTABLE
DIRECT-READING

Voltmeters
Millivoltmeters
Voltammeters
Ammeters
Millammeters

Ground Detectors and
Circuit Testers
Wattmeters
Ohmmeters
Portable Galvanometers



WESTON STANDARD PORTABLE DIRECT-READING VOLTMETER FOR DIRECT CURRENT

Our Portable Instruments are recognised as The Standard the world over. The Semi-Portable Laboratory Standards are still better. Our Station Voltmeters and Ammeters are unsurpassed in point of extreme accuracy and lowest consumption of energy

Weston Electrical Instrument Co.
114-120 William St., NEWARK, N. J., U. S. A.

A History of Physics

In Its Elementary Branches, including the Evolution of Physical Laboratories

By FLORIAN CAJORI, Ph.D.

Professor of Physics in Colorado College.

Cloth Cr 8vo, $1.60 *net.*

This brief popular History gives in broad outline the development of the science of physics from antiquity to the present time. It contains also a more complete statement than is found elsewhere of the evolution of physical laboratories in Europe and America. The book, while of interest to the general reader, is primarily intended for students and teachers of physics. The conviction is growing that, by a judicious introduction of historical matter, a science can be made more attractive. Moreover, the general view of the development of the human intellect which the history of a science affords is in itself stimulating and liberalizing.

"Prof. Cajori has followed his 'History of Mathematics' with a 'History of Physics' that is *even more interesting and valuable.* . . . Every teacher and student of physics should read it and add it to his library. It shows careful study, enthusiasm and a comprehensive grasp of the subject."—*Journal of Education.*

"A most valuable contribution to the subject."—*Scientific American.*

"Probably this will prove the most successful of all Prof. Cajori's histories."

—*The Nation.*

BY THE SAME AUTHOR

A History of Mathematics

Cloth 8vo, $3.50 *net.*

"What we have a right to expect in such a handbook is an agreeable narrative of the most material events in the history of mathematics, and this Professor Cajori incontestably supplies. The book was much wanted."—*The Nation.*

"A scholarship both wide and deep is manifest in this *History of Mathematics* which the author has infused with his own ardor in this department of science."

—*Journal of Education.*

"To the student with a love for mathematical science this book will be as entertaining as a romance."

—*The Transcript*, Boston.

A History of Elementary Mathematics

Cloth 12mo, $1.50 *net.*

"A most instructive, and at the same time a very readable piece of work, full of curious facts."—*The Bookman.*

"By no means an abridged edition of *The History of Mathematics*. It is an entirely new book, giving a somewhat detailed account of the rise and progress of Arithmetic, Algebra and Geometry. The book should be read by all teachers of these subjects, and by mathematical students generally."

—*American Mathematical Monthly.*

"The product of wide and scholarly research . . . For its historical facts and its suggestiveness, this work should be read carefully by all students and teachers of mathematics."—*The Dial*, Chicago.

THE MACMILLAN COMPANY, Publishers, New York

Recently Issued by The Macmillan Company

CAMPBELL—The Elements of Practical Astronomy
By W. W. CAMPBELL, Lick Observatory, California.
Cloth. 8vo. $2.00 *net*.

A new revised and considerably enlarged edition; comprehensive enough to include the principles which underlie all the varied work of a modern observatory.

CREW—The Elements of Physics
By HENRY CREW, PH.D., Professor of Physics in the Northwestern University. Cloth. 12mo. $1.10 *net*.

For the use of High Schools, etc.

HOSKINS—Elements of Graphical Statics
A TEXT-BOOK FOR STUDENTS OF ENGINEERING. By PROF. L. M. HOSKINS, Leland Stanford Jr. University. Illustrated. *New Edition Thoroughly Revised.* Cloth. 8vo. $2.25 *net*.

"The book is exactly what it purports to be—the elements of graphical statics. It is a well-written book for engineering students, the study of which will well repay them."—*Engineering and Mining Journal.*

RAYLEIGH—Scientific Papers
By JOHN WILLIAM STRUTT, Baron Rayleigh, D.Sc., F.R.S. *Professor of Natural Philosophy in the Royal Institution. Just ready.*
Vol. I. Cloth. 8vo. $5.00.

Containing miscellaneous mathematical and physical papers on vibration, radiation, electricity and magnetism, etc., etc.

WILLSON—Theoretical and Practical Graphics
An educational course in the theory and practical applications of descriptive geometry and mechanical drawing, prepared for students in general science, engineering or architecture. By FREDERICK NEWTON WILLSON, C.E. (Rensselaer), A.M. (Princeton), Professor of Descriptive Geometry, etc., Princeton University, Mem. Am. Soc. Mechanical Engineers. Complete. 4to. Cloth. $4.00 *net*.

Practical Engineering Drawing and Third-angle Projection. For Students in Scientific, Technical, and Manual Training Schools, and for Engineering and Architectural Draughtsmen, Sheet Metal Workers, etc. 4to Cloth. pp. 178, with 270 illustrations and 65 alphabets, etc.
Being a part of the above. Price, $2.80 *net*.

Some Mathematical Curves and their Graphical Construction. A Brief Treatise on the Properties, Methods of Construction, and Practical Applications of Conic Sections, Trochoids, Link-motion Curves, Centroids, Spirals, the Helix, and other Important Curves. (Part III) $1.50 *net*.

Shades, Shadows, and Linear Perspective. For Students of Engineering or Architecture, Professional Draughtsmen, etc. (Part V) $1.00 *net*.

Descriptive Geometry—Pure and Applied, with a chapter on Higher Plain Curves and the Helix; a theoretical and practical treatise prepared for courses in general science, engineering and architecture. (Part VI) $3.00 *net*.

Send for the Announcement List, and the List of Books,
Issued in 1898, by

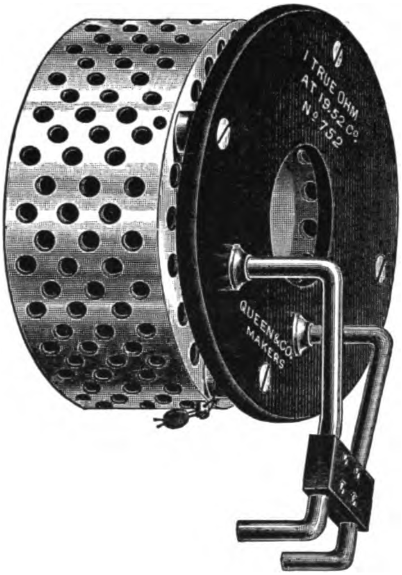
THE MACMILLAN COMPANY, New York

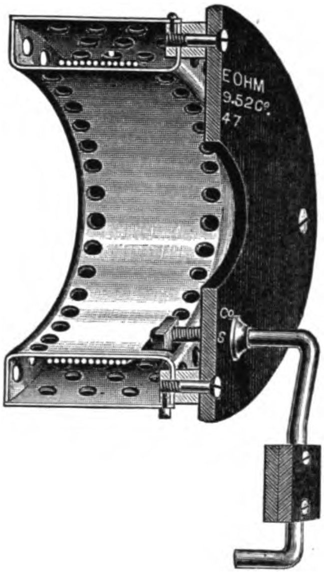
QUEEN

IMPROVED STANDARDS

Perfect circulation and
Uniform temperature
Guaranteed accuracy

$\frac{1}{50}$ of **1%**





$\frac{1}{50}$ of **1%**

Write for Circular No. 494.
We are offering the
above standards at about
half the usual price.

QUEEN & CO., INC.

J. G. GRAY, Pres.

'Electrical and Scientific Instruments

NEW YORK BRANCH
59 Fifth Avenue

1010 CHESTNUT ST., PHILADELPHIA.

Volume X. *February, 1900.* *Number 2.*

THE

PHYSICAL REVIEW.

THE INFLUENCE OF ELECTRIFICATION UPON THE SURFACE TENSION OF WATER AND MERCURY.[1]

BY ERNEST MERRITT AND SAMUEL J. BARNETT.

THE investigations of Exner and Tuma,[2] Warburg,[3] G. Meyer,[4] Luggin,[5] S. Meyer,[6] Nernst,[7] Behn,[8] S. W. J. Smith,[9] and many others have established beyond reasonable doubt that the electrocapillary phenomena occuring at the interface between a liquid metal and an electrolyte, in a capillary electrometer, for example, are due almost entirely to true alterations of surface tension resulting from the chemical changes of electrolysis. There are many other electrocapillary phenomena, however, which are undoubtedly caused largely, if not wholly, by the electrostatic forces that are developed by the presence of a charge upon the liquid surface. Thus when electrified, a soap bubble expands, a globule of mercury becomes more flattened, the wave pattern of a liquid jet issuing from an elliptical aperture becomes more elongated,[10] etc., etc. In such cases the effect of electrification is usually similar to that which would be produced by a *diminution* of the surface tension. This fact is probably due to the universal employment of convex surfaces in such experiments; in the case of a concave charged surface a consideration of the electrostatic forces would lead us to expect an apparent increase in the strength of the surface film.

---

[1] A paper read before the American Physical Society, December 28, 1899.

[2] Wien. Ber. 97 IIa, p. 923, 1888.

[3] Wied. Ann. 38, p. 321, 1889; ibid., 41, p. I, 1890.

[4] Wied. Ann 45, p. 508, 1892; ibid., 53, p. 845, 1894; et al.

[5] Wien. Ber. 102 IIa, p. 913, 1893.

[6] Wien. Ber. 105 IIa, p. 139, 1896.

[7] Zeitschrift für Elektrochemie, 4, p. 29, 1897.

[8] Wied. Ann. 61, p. 1897.

[9] Proc. R. S. L. 64, p. 253, 1899.

[10] Phil. Mag. Sept. 1882, p. 186.

To determine whether, in addition to this electrostatic effect, there exists a true effect of electrification upon the surface tension of a liquid, investigations must obviously be made by methods which make the calculation of the purely electrostatic effect feasible, so that it may be eliminated from the total effect. Several earlier investigations bearing upon the existence and magnitude of a true change in surface tension produced by electrification, although quite inconclusive so far as such an effect is concerned, were reviewed by one of us in a recent paper.[1] This paper also described a new and more elaborate investigation of the same problem, and showed that the effect in question, if it exists at all, is much smaller than previously supposed. As the theory of the method used had not been fully developed at the time of publication, no more definite conclusion could then be drawn. To complete this theory, and by means of it to give the observations their final discussion, is the object of the present communication.

For a detailed account of the experiments, reference must be made to the paper already mentioned. It is sufficient to state here that the tension of the liquid surfaces, uncharged and charged to known electrical densities, was investigated by the method of capillary ripples, in a form capable of giving the surface tension of unelectrified water with an accidental error not greater than 0.05%.[2] When the liquids were electrically charged, either positively or negatively, the velocity of the capillary waves was found to be less than when the surface was unelectrified. In the case of water this observed change would correspond to a diminution in surface tension of about 4% for a surface density of 1.75 electrostratic units. For mercury the apparent change was not so great, being slightly less than 1% for a surface density of 2.33. It is to be observed, in accordance with what has been said above, that the results cited do not necessarily prove the existence of a *real* change in surface tension; for the electrostatic forces acting upon a charged surface would cause a diminution in the velocity of capillary ripples even if the surface tension remained unaltered. In order to determine whether a real change in surface tension results from electrification, we must take account of the electrostatic forces in deriving the relation between surface tension and velocity.

---

[1] Barnett, PHYSICAL REVIEW, 6, p. 257, 1898.

[2] Barnett l. c., p. 263.

To determine the velocity of capillary ripples we have the general equation[1]

$$\frac{\delta p}{\rho} = -gh - \frac{d\varphi}{dt} \tag{1}$$

where ρ is the density of the liquid, h the displacement of the point considered above its equilibrium position, δp the resulting increment of pressure at the surface, φ the velocity potential, and g the acceleration of gravity.

If δp is due to capillarity only, we have

$$\delta p = -T\frac{\partial^2 h}{\partial x^2}$$

T being the surface tension; and equation (1) becomes

$$\frac{T}{\rho}\frac{\partial^2 h}{\partial x^2} = gh + \frac{d\varphi}{dt}\,. \tag{2}$$

For the case of plane sine waves such as we have supposed, this gives for the velocity v

$$v^2 = \left(\frac{g}{k} + T\frac{k}{\rho}\right)\tanh kl$$

l being the depth of the liquid, λ the wave-length, and $k = \frac{2\pi}{\lambda}$.[2]

In the experiments with which we are concerned the value of l was such that tanh kl differed from unity by less than one part in 2000, and we may put

$$v^2 = \frac{g}{k} + T\frac{k}{\rho}$$

with an error less than the experimental error.[3]

---

[1] Rayleigh, Treatise on Sound, Vol. II, § 353, Second edition.

[2] Rayleigh, Treatise on Sound, Vol. II, § 353, Second edition.

[3] Barnett *l. c.*, p., 263.

If the wave frequency is n we have $v = n\lambda$, and the final expression for the surface tension becomes

$$T = \frac{\rho\lambda^2}{2\pi}\left(\lambda n_2 - \frac{g}{2\pi}\right). \tag{3}$$

When the surface of the liquid is charged, however, a pressure $-\frac{2\pi\sigma^2}{K}$ is developed in addition to that due to capillarity, σ being the electric surface density and K the dielectric constant of the medium above the waves. In order to obtain the whole increment of pressure, δp, resulting from the displacement h, it is therefore necessary to add to $-T\frac{\partial^2 h}{\partial x^2}$ a term $-\frac{2\pi}{K}(\sigma^2 - \sigma_0^2)$ where σ_0 is the surface density of the charge on the undistorted surface. In the place of equation (2) we have therefore, in this case,

$$\frac{T}{\rho}\frac{\partial^2 h}{\partial x^2} + \frac{2\pi}{K\rho}(\sigma^2 - \sigma_0^2) = gh + \frac{d\varphi}{dt}. \tag{4}$$

The surface density σ at any point of the wave surface is a function of h whose form remains to be determined.

In the experiments already mentioned the surface of the liquid was made one coating of an air condenser, the other coating being formed of tinfoil supported on glass at a height of about 35 mm. above the surface. This arrangement was adopted in order to obtain a more intense charge upon the liquid surface, and to make the electrical conditions definite, so that all the quantities involved should be capable of exact determination. To find the distribution of the charge over the wave surface under the conditions of the experiment, we may proceed as follows:

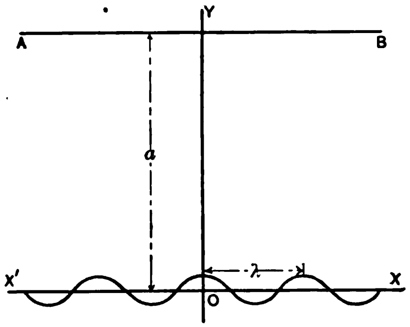


Fig. 1.

Let the axis of X be taken in the original level surface of the liquid, while the axis of Y is normal to the surface. Consider a train of plane waves of amplitude b travelling in the direction of X. As the waves proceed a rearrangement of the charge occurs; but this takes place so rapidly that the electrical conditions are always the same as though the ripples were stationary. The surface being at a potential V_0, while the plate AB is grounded, we have to determine the value of σ for all points along XX'.

For the region between the plates AB and the wave surface XX', the electrical potential V is a function of x and y subject to the condition

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} = 0. \tag{5}$$

It is clear that V, as a function of x, is periodic in the distance λ. With the origin as shown in the figure we may therefore write

$$V = A_0 + A_1 \cos kx + A_2 \cos 2kx + \cdots. \tag{6}$$

Since V must have the same value for $-x$ as for $+x$, none but cosine terms will appear in the expansion. The coefficients A_0, A_1, etc., are functions of y, but are independent of x.

Upon substitution in (5) we obtain

$$\frac{\partial^2 A_0}{\partial y^2} + \left(\frac{\partial^2 A_1}{\partial y^2} - k^2 A_1\right) \cos kx + \left(\frac{\partial^2 A_2}{\partial y^2} - 4k^2 A_2\right) \cos 2kx + \cdots = 0.$$

Since this equation holds for all values of x the coefficients must vanish separately. Therefore,

$$\frac{\partial^2 A_0}{\partial y^2} = 0, \quad \frac{\partial^2 A_1}{\partial y^2} - k^2 A_1 = 0, \text{ etc.}$$

The solution of these equations leads to the following values for the coefficients in (6):

$$\begin{aligned} A_0 &= M_0 y + N_0 \\ A_1 &= M_1 \varepsilon^{ky} + N_1 \varepsilon^{-ky} \\ A_2 &= M_2 \varepsilon^{2ky} + N_2 \varepsilon^{-2ky} \\ &\text{etc.} \end{aligned}$$

The constants of integration are to be so chosen as to satisfy the boundary conditions, viz.: (1) For $y = a$, $V = 0$ for all values of x: (2) for $y = b \cos kx$, $V = V_0$.

The first condition requires that A_0, A_1, etc., should separately vanish for $y = a$. Therefore

$$M_0 = -\frac{1}{a} N_0$$

$$M_1 = -N_1 \varepsilon^{-2ka}$$

$$M_2 = -N_2 \varepsilon^{-4ka}$$

etc.

For the coefficient A_1 we therefore have

$$A_1 = N_1 \left(\varepsilon^{-ky} - \varepsilon^{-2ka+ky}\right).$$

Since the expression for V is to be used only in order to find the force at the surface of the liquid, and from this the value of σ, we are chiefly concerned with its value for points immediately above the surface. For such points y is small when compared either with a or with λ. The exponent of the second term of A_1 for points in the immediate neighborhood of the surface is therefore practically equal to $-2ka$. In the experiments upon water the value of a (distance between the liquid surface and the plate above) was about 4 cm., while λ was always less than 0.5 cm. In the experiments upon mercury, the value of a was about 3.4 cm., and that of λ less than 0.37 cm. In all cases the quantity $2ka$ is therefore greater than 100, and the second term is the expression for A_1 is entirely negligible. This is also true of the corresponding terms in A_2, A_3, etc. The expression for V therefore takes the form

$$V = -N_0 \frac{y}{a} + N_0 + N_1 \varepsilon^{-2ky} \cos kx + N_2 \varepsilon^{-2ky} \cos 2kx + \cdots. \quad (7)$$

For points on the wave surface, where $y = b \cos kx$, the potential is V_0. Therefore for points on the surface

$$V_0 = N_0 - N_0 \frac{b}{a} \cos kx + N_1 \varepsilon^{-ky} \cos kx + N_2 \varepsilon^{-2ky} \cos 2kx + \cdots.$$

Remembering that the amplitude, b, of the ripples is extremely small, and that the y in the above expression can in no case exceed b, it is clear that the exponentials are practically equal to unity; and we have as a close approximation,

$$0 = N_0 - V_0 + \left(N_1 - \frac{b}{a} N_0\right) \cos kx + N_2 \cos 2kx + \cdots.$$

Since this relation holds for all values of x, the coefficients $N_2\,N_3$, etc., vanish, and

$$N_0 = V_0, \quad N_1 = \frac{b}{a} V_0.$$

The final approximate expression for V is therefore

$$V = V_0\left(1 - \frac{y}{a} + \frac{b}{a}\epsilon^{-ky} \cos kx\right) \tag{8}$$

and that for Y, the vertical component of the force at the surface, is

$$Y = -\frac{\partial V}{\partial y} = \frac{V_0}{a}(1 + kb \cos kx).$$

For waves of the extremely small amplitude used the horizontal component of the force is neglible in comparison with Y. Therefore,

$$\sigma = \frac{KV_0}{4\pi a}(1 + kb \cos kx).$$

Remembering that $\frac{KV_0}{4\pi a} = \sigma_0$, that $kb \cos kx$ is small, and that $b \cos kx = y = h$, we have, finally,

$$\sigma^2 - \sigma_0^2 = 2kh\sigma_0^2 = \frac{4\pi\sigma_0^2}{\lambda} h$$

and equation (4) becomes

$$\frac{T}{\rho}\frac{\partial^2 h}{\partial x^2} = \left(g - \frac{8\pi^2\sigma_0^2}{K\lambda\rho}\right)h + \frac{d\varphi}{dt}. \tag{9}$$

Upon comparing this with equation (2) it appears that the effect of electrifying the surface is to alter the velocity of the waves in the same sense and to the same extent as would a diminution of the acceleration of gravity by $\frac{8\pi^2\sigma_0^2}{K\lambda\rho}$.

The corrected expression for the surface tension [see (3)] is, therefore,

$$T = \frac{\rho\lambda^2}{2\pi}\left(\lambda n^2 - \frac{g}{2\pi}\right) + \frac{2\lambda\sigma_0^2}{K}. \tag{10}$$

The *apparent* surface tension *i. e.*, the value of T obtained from (3) [or from (10) without taking into account the final term, which gives the correction for electrostatic influences] is thus always less than the true surface tension by the amount $\frac{2\lambda\sigma_0^2}{K}$.

TABLE I.

Water.

V	λ	σ_0	$\frac{2\lambda\sigma_0^2}{74.3}$	T Apparent.	T Corrected.
0	0.496	0.00	0.00%	100.00%	100.00%
9000	.496	0.58	0.45	99.61	.06
12000	.495	0.77	0.79	99.29	.08
15000	.495	0.97	1.25	98.98	.23
18000	.494	1.17	1.82	98.37	.19
21000	.493	1.36	2.46	97.69	.15
24000	.492	1.56	3.22	96.94	.16
27000	.491	1.74	3.00	96.13	.13

TABLE II.

Mercury.

V	λ	σ_0	$\frac{2\lambda\sigma_0^2}{403.4}$	T Apparent.	T Corrected.
0	0.369	0.00	0.00%	100.00%	100.00%
9000	"	0.70	.09	99.92	.01
12000	"	0.93	.16	.88	.04
15000	"	1.17	.25	.83	.08
18000	"	1.39	.35	.75	.10
21000	"	1.63	.49	.61	.10
24000	"	1.87	.64	.44	.08
27000	"	2.09	.80	.25	.05
30000	"	2.33	.99	.08	.07

The experimental values of this apparent tension of water and mercury have been given for a number of different values of σ_0 in Tables IV. and V. of the paper referred to above, expressed in percentage of the values for $\sigma_0 = 0$.[1] By the use of equation (10) we are now able to apply the correction due to electrostatic influences. The results are given in Tables I. and II., expressed as before in percentage of the maximum values of T. These maximum values were $74.3 \frac{\text{dynes}}{\text{cms}}$ for water and $403.4 \frac{\text{dynes}}{\text{cms}}$ for mercury, respectively, which were obtained by making the following substitutions in (10):

---

[1] Barnett, l. c., pp. 281–282.

$$K = 1, \quad g = 981, \quad n = 64.4, \quad \sigma_0 = 0$$

also, for water

$$\lambda = 0.496, \quad \rho = 0.998$$

and for mercury

$$\lambda = 0.369, \quad \rho = 13.54.$$

The final column of Table I. gives for the corrected surface tension of an electrified water surface values ranging from 0.06% to 0.23% higher than that of the uncharged surface, the average value being about $\frac{1}{7}$% higher; while the corresponding column of Table II. gives for the tension of an electrified mercury surface values from 0.01% to 0.10% higher than that for no electrification—the mean being about $\frac{1}{16}$% higher. The fact, however, that the very small variation follows no law in either case, being often less for high than for low electric surface densities, would indicate that it possesses no significance, and is due wholly to experimental errors. To make this evident, we shall examine the accidental error in measuring the "apparent" surface tension of water; what is said of the experiments upon water being true, with little alteration, for those upon mercury. As was shown in the paper already often quoted, a given percentage error in measuring λ produces in the computed value oi T, under the conditions of the experiments, a percentage error about three times as great. More accurately, $\frac{\Delta T}{T} = 2.9 \frac{\Delta \lambda}{\lambda}$. An examination of figure 6 (*l. c.*) shows that in measuring λ the average departure from the mean (given by the curves) in each of the two measurements necessary was about $\frac{1}{20}$%. The average accidental error in T is therefore about $\frac{1}{4}$%; so that the small differences in the corrected value of the surface tension are all within experimental errors, and we may safely conclude that no effect of electrification upon the surface tension has been observed.

The complete investigation, therefore, while not excluding the possibility of discovering an effect of electrification upon surface tension by the use of improved methods and still more intense charges, itself gives no support to the view that such an effect exists.

AN OBJECTIVE REPRESENTATION OF THE HYSTERESIS OF IRON AND STEEL.[1]

KNUT ÅNGSTRÖM.

THE very happy thought of using cathode rays for the study of alternating currents and of alternating magnetic fields appears to have had its origin with Mr. Braun.[2] The method employed is obvious from Fig. 1, which shows a tube of the kind furnished by Geissler in Bonn, according to Braun's specifications. Cathode rays from the terminal *K* pass through a small hole in the diaphragm *D*, and produce a brilliant spot of light upon that portion of the screen *S* upon which they fall. This screen, which is covered with phosphorescent material, fills the end of the tube. The approach of a magnetic pole at *D* deflects the spot of light. If a small magnetizing coil, through which alternating currents flow, be fastened beneath the diaphragm, the spot of light is thrown into a vibratory motion which is capable of analysis by means of the revolving mirror. If two coils, placed perpendicular to each other, with their axes at right angles to the axis of the tube are fastened in the neighborhood of the diaphragm, and each of these coils be supplied with alternating current, the spot of light under the influence of the two forces is given a combined motion analagous to Lissajous's curves.

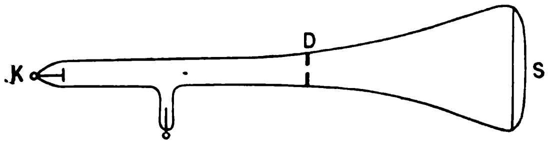


Fig. 1.

---

[1] Translated from the transactions of the Royal Academy of Sciences, Stockholm, 1899, No. 4, by E. L. N.

[2] Braun. Wiedemann's Annalen, 60, p. 552, 1897; Elektrotechnische Zeitschrift 19, p. 204, 1898. The latter article is known to me only through an abstract in Wiedemann's *Beiblätter*, 22, p. 884, 1898.

Braun, in addition to his other experiments, has investigated the velocity of the transmission of magnetization in iron. Since these experiments bear a certain relation to some of those which follow, I take the liberty of describing them in a translation of his own words.[1]

"An iron rod, 2 m. long and 9 mm. in diameter, lies in a horizontal position, perpendicular to the axis of the tube, its end as near as possible to the diaphragm. Along this rod, a small magnetizing coil may be moved. A second coil precisely similar to this, is arranged in a vertical plane, likewise perpendicular to the tube, so that under the simultaneous action of an alternating current in the two spools, the spot of light describes a curve, the form of which is determined by the pole lying nearest to the diaphragm. Both coils are traversed by the same alternating current.

"If the coil is moved along the rod, the shape and position of the ellipse of vibration changes and when the middle of the spool is about 42 cm. from the end of the rod a difference of phase of $\frac{\pi}{2}$ shows itself which is independent of the strength of the current. From the number of oscillations (50) of the alternating current it follows that the velocity of 'propagation' of magnetic excitation is eighty-six $\left(\frac{\text{meters}}{\text{seconds}}\right)$, a value which accords very well with that found by Oberbeck under similar conditions (88.7 meters for a rod 8.7 mm. in thickness, with a vibration frequency of 133)."

As Braun himself remarks, we have to do here with complicated phenomena, and it is a question whether the value of the velocity of transmission thus determined is really that of the magnetic excitation.

While engaged in repeating some of Braun's experiments, I asked myself whether it would not be possible by means of his apparatus to represent objectively the hysteresis curves in iron and steel and to exhibit the same, and after several preliminary attempts I found a simple arrangement which led, in a most beautiful manner, to the result. If a piece of iron or steel is magnetized in a magnetic field, for example in a long magnetizing coil, and if we plot a curve with rectangular coördinates in which the strength of the field H is taken as abscissa and the induced magnetism I as ordinate, we obtain, as is well known (when H passes through values from H_1 to $-H_1$), a curve which we call the hysteresis curve, and which owes its existence to the fact that induced magnetism in iron does not possess the same value when increasing and when decreasing for equal values of H. This curve and the surface enclosed by the same, which represents a certain amount of energy consumed in the cycle of magnetization, may be determined in a variety of ways and when changes in the magnetizing force take place slowly the process is one which offers no great difficulties. The relations are, however, much more complicated when the intensity of the field changes rapidly.

---

[1] Wiedemann's Annalen, 60, p. 557.

In the latter case losses of energy occur, as is well known, not only in consequence of the actual phenomena of hysteresis, but also in consequence of the action of Foucault currents. These which are generated in part by the inducing current in the mass of the iron, and in part by the induced magnetism itself, cause a retardation of the magnetic action upon the deeper layers of the substance (electro-magnetic screening). Attempts to separate these various factors from one another and to study them in detail have been made in recent times, among others by Ch. Maurain,[1] and by Wien.[2]

From their work one can form a clear conception of the difficulties with which such investigations are surrounded, and of the advantage of finding a simple method which would allow this relationship to be easily determined, which would make it possible to gain a ready acquaintanceship with the subject, and which, in other ways, would furnish a certain check upon the results. This has been attempted by Ewing,[3] who has constructed an apparatus that records the curves of magnetization automatically. In this apparatus a wire, which is placed between the poles of a permanent magnet, is traversed by a magnetizing current. Another wire carries a constant current and the latter is subjected to the action of the induced magnetism in the iron. The forces that act upon these wires turn a little mirror about two axes situated at right angles one to another. When the iron passes through a complete cycle of magnetization, a beam of light reflected from the mirror records upon a photographic plate the curve of magnetization. These and similar methods can, in the nature of the case, only be used where the changes in the magnetizing force take place slowly, since, as is the case with all mechanical devices, the inertia of the many parts introduces disturbances whenever the period of vibration is rapid.

---

[1] Maurain; Annales de Chimie et de Physique; Series 6, 14, p. 208 (1898).

[2] Wien; Wiedemann's Annalen; 66, p. 859 (1898).

[3] Ewing; The Electrician; May, 1893.

First arrangement of my apparatus.—The first arrangement which I used will be easily seen from Fig. 2. The four coils, S, S_1, M, M_1, are fastened around the diaphragm in the manner shown. The dimensions of the coils are as follows:

S, length 2 cm., external diameter 1.6 cm., internal diameter 0.5 cm., diameter of wire 0.3 mm.

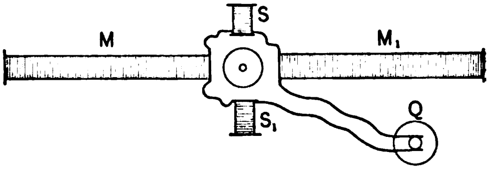


Fig. 2.

M, length 20.5 cm., number of turns, 30 per second.

The coils, S and S_1 were wound upon wood, M and M_1 upon glass tubes. The current from a source Q passes through these four coils in series. It flows through S and S_1 (the indicator coils of Braun), in such a manner that the magnetic action of the same at the opening of the diaphragm is additive; through M and M_1 in such directions that their action at the point is annulled. The horizontal deflection produced by means of the action of the current in S and S_1 is proportional to the strength of the current and also to the magnetizing force. If a rod of iron be introduced into one of the magnetizing coils, the spot of light will be deflected in a vertical direction with a force which is proportional to the induced magnetism, so that under these circumstances the spot of light must pass through a true hysteresis curve.

Second arrangement.—The two indicator coils S and S_1 retain their places, but they are slightly displaced backwards and forwards respectively, with reference to the plane of the diaphragm. The magnetizing coils M and M_1 are placed parallel to each other and at equal distances from the diaphragm as shown in Fig. 3. The action of the current and of the magnetic field is the same as in the first arrangement. When currents of rather high intensity are used, it is easily possible to follow the movement of the spot of light directly with the eye. If, on the contrary, the cycle of magnetization is gone through with slowly, and especially if it is desired to measure the curve of hysteresis with precision, a photographic record is very advantageous. I set up the photographic camera in such a way that the images of the curves should be taken in half size. An exposure of from ten to twenty seconds suffices for the production of a good negative, which when developed and fixed, can be enlarged to any desired size by projection. The figures in the accompanying plate are reproduced directly from such negatives. The curves shown in the plate are from experiments upon hard steel wires 10 cm. in length and 0.175 cm. in diameter. I. was obtained from a wire containing 0.1 per cent.; II. from a wire containing 0.4 per cent. and III. from a wire containing 0.9 per cent. of carbon. I.–II., I.–III. and II.–III. are "difference curves" between these (I., II. and III.) taken two and two.

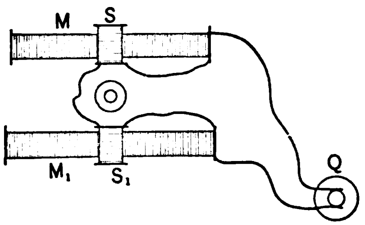


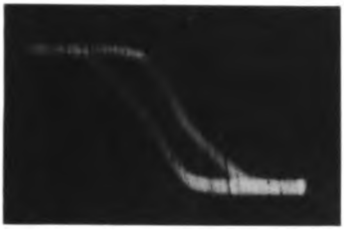
Fig. 3.

For very slow cycles, of magnetization (to produce static curves of hysteresis) I used as a source of current a storage battery, and varied the strength of the current by the introduction of liquid resistance, so as to change the magnetizing force between the desired limits. Alternating current was produced from a small hand-driven Siemen's alternating dynamo (from Ducretet in Paris) with which it was possible to vary the number of alternations between twenty and sixty per second. In this paper only a few examples of these experiments are given. I hope later to return to a more complete description of the results. Figs. 4–8 show drawings from some of the photographic records obtained with the second arrangement of the apparatus. The curves marked *A*, *B*, *C*, *D* refer to the following samples of iron:

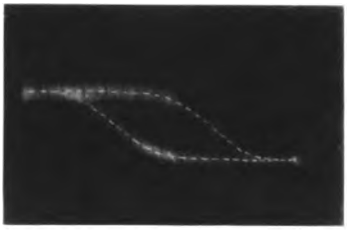
(*A*) A rod of iron; length 10 cm., diameter 0.3 cm., amount of carbon 0.2%; tempered. (From Bofors in Sweden.)

(*B*) A rod of iron; length 10 cm., diameter 0.3 cm., amount of carbon 0.8%; hardened. (From Bofors in Sweden.)

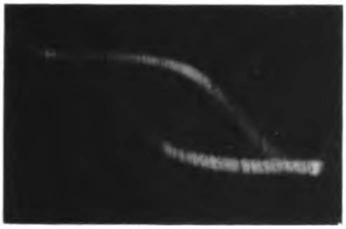
(*C*) A bundle of very soft tempered iron wire, length 10 cm., diameter of each wire 0.082 cm.



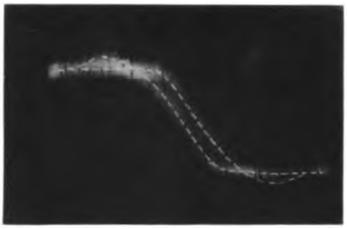
I



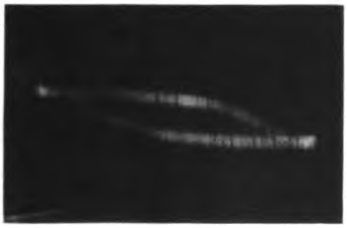
I–II



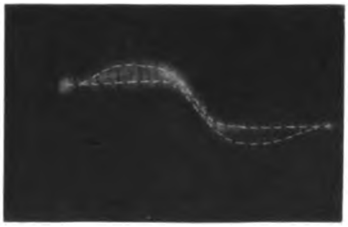
II



I–III



III



II–III

ÅNGSTRÖM: HYSTERESIS OF IRON AND STEEL.

(*D*) A precisely similar bundle of wires, surrounded by a tube of brass. External diameter of the tube 0.48 cm., internal diameter 0.30 cm.

Figure 4 shows the static curve of hysteresis. Figure 5 the corresponding curve for alternating current of about twenty reversals per second. Figure 6 shows the same curve for about sixty reversals per second. The maximum strength of the current in these experiments was maintained as nearly as possible constant, with a value of two amperes. In the static hysteresis curves obtained by this method, the influence of hardness is easily recognized. In the case of very soft iron there is naturally no difference between a massive rod and a bundle of wires. In massive rods of soft iron with increasing frequency of alternations, the loops change greatly but in the case of hard iron, the change is insignificant. That these

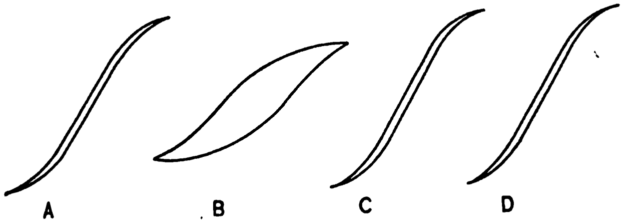


Fig. 4.

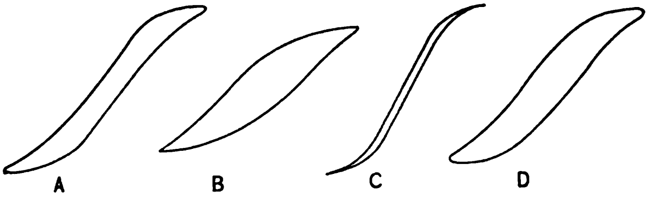


Fig. 5.

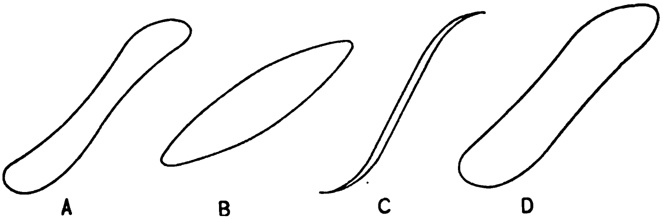


Fig. 6.

changes are almost exclusively due to the Foucault currents is evident from the experiments with the bundle of wires with and without the brass tube. The change in the loop with the number of alternations could not be detected in the case of the bundle, without the brass tube, in which case Foucault currents are almost completely excluded, but when the bundle was surrounded by the brass tube, the change in the loop was almost as great as in the case of a soft rod of massive iron.

In general, these results are in good agreement with the experiments of Maurain and of Wien in so far as they are capable of comparison with those investigations. Wien has used alternations up to a frequency of five hundred and twenty, and has studied the changes in the real hysteresis curves. Maurain finds that in the case of bundles of very fine wire, the loss of energy in the cycle of magnetization up to periods of fifty-five alternations per second is not appreciably dependent upon the frequency.[1] Unfortunately, I have not as yet had the means at my disposal to extend these investigations to a very high frequency.

A very interesting modification of the above mentioned experiment can be made by providing the magnetizing coils M and M_1 with iron cores. The curves obtained in this way are difference curves, which depend upon the varied permeability and hysteresis of the specimens, as well as upon the strength and difference of phase which the Foucault currents in the same possess. Figures 7 and 8 afford examples of such difference curves for two different frequencies, obtained by the used of the two rods, A and B, just described. In this case the difference curve for low frequencies is principally due to the difference in hysteresis. At higher frequencies, we have to do, in addition to the above, with unequal changes of phase of the Foucault currents. The curves in figure 8 were

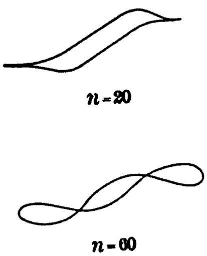


Fig. 7.

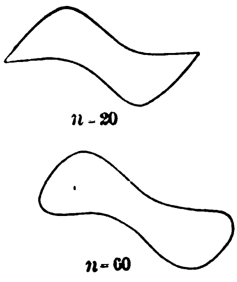


Fig. 8.

---

[1] Maurain, l. c., p. 279.

obtained by means of the bundles of wires, C and D (with and without the brass rod). In this instance, the hysteresis remains unchanged, and the difference curve is to be ascribed exclusively to the action of the Foucault currents. This last named arrangement of the experiment affords a simple and practical method of testing various samples of iron in respect to their magnetic properties, and likewise to the amount of carbon which they contain.[1] If we place in the magnetizing coils two precisely similar rods of iron of the same character, they neutralize each other and the spot of light moves in a horizontal straight path just as if no iron were placed in these coils. If the rods, however, possess different properties, we obtain more or less well marked difference curves. If, therefore, we have samples of iron, the properties of which are precisely known, we can compare with them directly the property of unknown samples of iron of the same dimensions. The usefulness of this application of the method in electro-technical work, and particularly in the construction of dynamo machines, where hysteresis is of great importance, is obvious.

If, in the first arrangement of the apparatus the rods of iron are brought with their ends very near the opening of the diaphragm and if the magnetizing coils in the second arrangement are fastened directly above and below the rods, and we compare the results obtained in these ways, with one another, it is found, particularly if the iron rods are of great length, that there is a very marked difference between the two curves. The hysteresis curves trend more slowly upward and downward in the first case than in the second. I was for a time at a loss to explain these facts, but I believe that the phenomenon is due to changes in the magnetic distribution within the rods with increasing magnetization. The following experiments confirm this opinion. If we make use only of the two long magnetizing coils, and arrange these as shown in Figure 9, we find when the diaphragm is at the point a, symmetrically located with reference to the two spools and when a bundle of iron wires is thrust into one of the tubes, that vertical vibrations take place. If now the diaphragm is pushed along to the point, b, c, etc., the direction of vibration tends to turn itself in such a manner as to be always perpendicular to the lines of the magnetic field. At the same time, we notice, however, that the line of vibration as is shown in Figure 9, becomes curved at the ends. If we replace the bundle of wires with an iron rod, we no longer get lines of vibration, but complete loops. With increasing intensity of magnetization, the system of lines of force is displaced, and the whole action is like a shifting of the poles towards the ends of the magnetized rod. If this shifting is not the same for increasing and diminishing magnetization, or in other words, it is connected with a hysteresis effect, we obtain the above described loops. Through this displacement of the poles, we may likewise explain the above difference in the curves obtained with the first and second arrangements of the apparatus.

---

[1] This comparison is based upon the assumption that the iron in other respects is of similar chemical and physical structure.

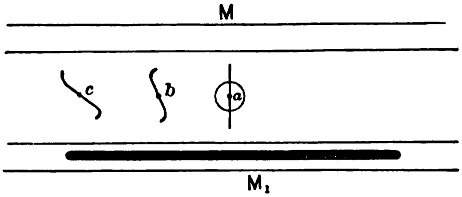


Fig. 9.

When short rods are used, and at a considerable distance from the diaphragm, the difference between the curves obtained is unimportant. The precise measurement of these curves would probably teach us much concerning the permeability and hysteresis of iron. Before entering upon this work in the case of certain varieties of Swedish iron, I purpose, however, to attempt important improvements in the cathode tube. I hope then to be able to work with currents of much higher frequency.

To Mr. Granqvist of the physical laboratory at Upsala, who assisted me in most of these experiments, I desire to express my heartiest thanks.

A DETERMINATION OF THE NATURE OF THE ELECTRIC AND MAGNETIC QUANTITIES AND OF THE DENSITY AND ELASTICITY OF THE ETHER, II.

REGINALD AUBREY FESSENDEN.

Summary.

IT was first shown that if we consider only those electric and magnetic phenomena which can be exactly expressed by equations containing only electric, magnetic and gravitational quantities, with those of length, mass and time, that there are an infinite number of theories possible.

If we suppose that the electric and magnetic quantities are such as we meet with in mechanics the number of possible theories will be much reduced.

We have no right to make this assumption, however, and the reduction cannot be regarded as rigorous, but rather as having a certain degree of probability or as serving as an indication, and hence can only be justified by showing that the results obtained by its means are in accordance with observed facts.

On this basis it was shown that one of two theories is the correct one, *i. e.*, that either x is a density and μ a compliancy or μ is a density and x a compliancy. It was then shown that if either coefficient is a compliancy, then, when the corresponding intensity is varied, the corresponding coefficient should vary in the opposite direction.

If neither theory were correct this change should not necessarily take place.

It was found that there is such a relation existing between one of the coefficients and the corresponding intensity, *i. e.*, between μ and H, whilst there is no such relation observable between the other coefficient and intensity.

It was then shown that the rate of change of the coefficient μ with the intensity H was exactly that called for by the theory and a most rigorous experiment showed no deviation from the rate required not within the limits of experimental error.

It was shown that this relation held not only for iron and other strongly magnetic substances, but also for a wide range of substances whose reluctivities varied from 0.0003 to 1000, and was hence probably a universal relation.

Corroborative evidence was then given, showing that the reluctivity varied in different materials, in the same materials under temporary stress and in the same materials under permanent strain, in such a way as to accord with the theory. It was also shown that there was a relation between the coefficients of reluctance and of hysteresis which was also in accordance with the theory.

It was then shown that there was evidence of such a connection between capacity and density as agreed with the theory.

From this last relation the density and elasticity of the ether were determined, to a first approximation.

CONCLUSION.

Having determined the qualities of the electric and magnetic quantities, we may next proceed to more precisely determine their nature. For convenience we will here reproduce equations 12–15.

(12) $Q = M/T,$
(13) $P = L^2,$
(14) $\varkappa = M/L,$
(15) $\mu = LT^2/M.$

The complete determination of these qualities is made as follows:

From equation 3, Maxwell's equation, we have

$$\varkappa^{1/2}\mu^{1/2} = T/L = 1/\text{velocity}.$$

Let us take the X axis as in the direction of the lines of electric flux, the Y axis as in that of the lines of magnetic flux, and the Z axis as perpendicular to both.

The velocity of light is therefore to be measured along the Z axis. We know also that in the expression for volume, *i. e.*, L^3, the three lengths are to be taken in three different directions.

We have therefore

$$M/L_xL_yL_z \times \mu = T^2/L^2,$$

whence

$$\mu = L_xL_yT^2/ML_z.$$

Again, we have

$$B^2/8\pi\mu = \text{work per c. c.} = (P/\text{area})^2/8\pi\mu.$$

This work is done in the Y direction,

hence

$$P = L_xL_y \text{ and } B = L_y/L_z.$$

From the equation

$$H\mu = B$$

we have

$$H = ML_y/L_yL_xT^2 = M/L_xT^2.$$

To recapitulate,

$$\begin{aligned} B &= L_y/L_z \\ H &= ML_y/L_yL_xT^2, \\ \mu &= L_xL_yT^2/ML_z. \end{aligned}$$

From this we see that a current flowing in a circuit (this being in the X axis) produces a twisting force around the axis of the wire, H being the intensity of the force, or the force per sq. cm. This force produces a shear, B being the amount of the shear, and the shear at any point depending upon the distance from the wire. The amount of the shear for a given force per sq. cm. depends upon the resistance of the medium to shearing stress, and $1/\mu$ is the rigidity modulus of the medium. Hence $B = H\mu$.

In the presence of iron the virtual rigidity is greatly reduced. The stress H produces a shear B_e in the ether numerically equal, if we take the permeability of the ether as unity, to H. The same stress also produces a virtual shear B_i, ($= 4\pi \times$ intensity of magnetization). The sum of these two shears makes up the total shear B, or $B = B_e + B_i$ (equals numerically $H + 4\pi I$).

The shear B_e increases directly as H. Since the greatest value of H which we can reach is not more than 100,000, which corresponds to a force of only about 4.10^8 dynes per cm. and the rigidity of the ether is, as we have found, approximately 6.10^{20}, we could hardly have expected it to be otherwise, as the greatest shear which we can produce is through but an infinitesimal angle.

The shear B_i (for it is virtually equivalent to a shear), increases as H increases, but reaches a limit, for iron, at about $B_i = 25{,}000$. It does not increase however, directly as H, but as we have seen the two are connected by the formula

$$B_i = H/(a + bH).$$

Under certain circumstances, for example with stressed nickel, as shown in Fig. 4, the constant a is reduced to zero and the intrinsic shear takes its full value with an indefinitely small value of the stress H, any additional increase in B being due entirely to increase of the extrinsic flux.

It will be noted that P/μ is a force in the Z direction.

Taking up next the electric quantities, from the formula

$$D^2/8\pi x = (Q/\text{area})^2/8\pi = \text{work per c.c.}$$

we get, since the work is done along the X axis,

$$Q = M/T$$

$$\text{and } D = M/L_yL_zT.$$

From the formula

$$QE = \text{work},$$

the work being done along the X axis,

$$E = L_x^2/T$$

$$F = L_x/T.$$

To recapitulate,

$$D = M/L_yL_zT$$

$$F = L_x/T$$

$$x = M/L_xL_yL_z$$

We may interpret this as meaning that a varying magnetic field produces a flow in the ether in a plane perpendicular to the direction of the magnetic lines, which flow persists, with varying velocity, so long as the amount of the shear is changing. The electric intensity F at any point is the velocity of the flow at that point. The capacity, x, is the density of the flowing medium and the displacement per square cm. D, is the current density.

If we have a condenser with a dielectric part paraffin, of density 0.9 and part vacuum, then since the electrostatic induction is continuous through the interface, *i. e.*, $M/L_y L_z T$ is constant, the velocities must be different for each side of the interface.

The density of the ether is 0.6, *i. e.*, there is that much inertia per c.c. in the vacuum. In the space occupied by the paraffin the total amount of inertia is $0.9 + 0.6 = 1.5$.

Therefore, since the flow per sq. cm. is constant, and the densities vary in the ratio 0.6 : 1.5, the velocities, *i. e.*, the electric intensities, must vary in the inverse ratio. Hence the slope of electric potential changes at the interface in the ratio 1.5 : 0.6.

But the energy is given by the formula $mv^2/2$. Therefore in the ether the energy per c.c. is 5/3 of that in the paraffin.

The velocity in the ether corresponding to any voltage may be calculated, on the assumption that all the ether flows. We have

$$xF^2/8\pi = \text{energy per c.c.}$$

and x is 0.6. If F is one electrostatic unit and x is taken as 1 for the ether, we have,

$$v^2 \times 0.3 = 1$$

and $$v = 1.8 \text{ cm. per sec.} = F/\sqrt{4\pi}.$$

Since a volt is 1/300 electrostatic unit, the velocity corresponding to a volt is 0.006 cm. per sec., or about 20 times the velocity of the hydrogen atom when electrolyzed in aqueous solution.

We might naturally look for an effect upon the velocity of light in the direction of the lines of electrostatic force, since F is a velocity. Such an effect was discovered by Kerr, who has shown (Phil. Mag., April, 1894) that only the component vibrating in the plane of the lines of force is changed in the phenomenon which he discovered, the component perpendicular to them being unaffected, but the phenomenon is complicated here by the presence of matter, the effect is so large that it cannot be due directly to the flow of the free ether, and does not vary directly as F.

Rayleigh has examined the effect when light is passed through a solution carrying a current, without observing any increase however, as might be expected, since one volt per cm. corresponds to a velocity of only 0.006 cm. per sec. and the velocity of light is 3.10^{10} cms. per sec.

We can, however, without any extraordinary difficulty, obtain a voltivity of 150,000 in a vacuum tube, and this would mean a velocity of 800 cms. per sec., which, I learn from my colleague, Professor Wadsworth, should be observable with the refractometer. The experiment will be made.

It may be mentioned here that in doubly refractive substances the piezo-electric effect along different axes should vary with the refractive indices.

To consider next electromagnetic phenomena. We must first find the quality of the curl from the following two equations, representing the effect of a varying electrostatic flux or an electric current, and of a varying magnetic flux:

$$\text{curl}_x\, Q/T = G = H \times L_y \text{ and } \text{curl}_y\, P/T = E = F \times L_x$$

whence

$$\text{curl}_x \text{ (curl round } X \text{ axis)} = L_y/L_x$$

$$\text{curl}_y \text{ (curl round } Y \text{ axis)} = L_x/L_y.$$

It is first to be noticed that a moving flux and a current are not identical in effect. A rotating magnetized ring produces no voltage of the first order of effect in a coil surrounding it. Nor can a rotating glass ring with an electrostatic flux passing round the torus, magnetize to any degree an iron ring encircling it. The movement of the electric lines in a lateral direction is what gives the gilbertance and the lateral movement of the magnetic lines the voltage. Longitudinal motion of the flux has no effect, as there must be a change in the amount of the enclosed flux.

Just outside a wire carrying a current the lines of voltage are almost normal to the surface of the wire, as was pointed out by Heavyside, who discovered the true method of establishment of current in a wire. So long as the ends of the lines are stationary with reference to the wire, there is no current and no magnetic force. But when they move, the original velocity along the lines has compounded with it the lateral velocity of the lines, and the acceleration of the ether measures the value of H at any point. When we establish an electrostatic field between the plates of a condenser, the flow between the plates causes, in some manner, a tension normal to the surface of the plates and a lateral pressure. Whilst charging the plates we are increasing the lateral pressure. This creates a shear in a direction at right angles to both tension and pressure, just as, in thrusting a ball sidewise between two other balls, the side pressure thrusts the two balls apart and a force at right angles to the lateral pressure is thus produced.

To some extent the above description is founded upon assumptions. For example, it does not follow that because F is a velocity and because it is the velocity of the ether (since we can have a value of F in a vacuum), that all the ether in a cubic centimeter of space is moving when we have a voltage there. This however does not affect the *nature* of the quantity but only the configuration.

I have not suggested any detailed theory of the configuration, for the reasons given in the preface. There are a number of experiments to be made before we can go further with any degree of certainty. Any detailed work on configuration would imply a theory of the nature of matter, which is foreign to my subject. We may of course choose some configuration to work with. In doing this the first thing to be looked after is to have the flux of ether between the two charged plates of a condenser return again, as in a closed circuit. True there is no real reason for this. Continuous flow, without return, is inconceivable, but that is not a very serious objection.[1] To avoid having sinks, however, we may choose some suitable configuration. Possibly the best available is that given by Fitzgerald in his paper "On a Hydrodynamical Hypothesis as to Electromagnetic Actions."[2] According to this, the electrostatic line is a spiral vortex. This gives us our tension between the plates, and the lateral pressure. It can also give us the magnetic shear, and in the paper referred to it is shown that the equations for wave propagation and energy in such a medium as described are identical with those of light. We may also take the atom itself as a spiral vortex, and can get a secondary stress, depending upon angular velocity, to account for gravitation, which would be the gravitational stress. As we have found the rigidity of the ether to be

---

[1] It is not many years since one of our greatest and most esteemed philosophers (Herbert Spencer, First Principles) came to the conclusion that the idea of an indivisible atom was inconceivable. As a matter of fact it *was* inconceivable then, but it is so no longer. One is reminded of Beaconsfield's witty note to his allegorical history of England (Isle of Fantasia, Chapter IV.) "First principles are the ingredients of positive truth. They are eternal and immutable, as may be seen by comparing the first principles of the eighteenth century with the first principles of the nineteenth."

[2] Royal Dublin Soc., December 12, 1898.

6.10^{20} this would permit of even the large values which Maxwell has pointed out are necessary with a stressed medium theory of gravitation.

Under these circumstances we might expect the permeability of a material to vary with a change in the force of gravity, and a charged condenser to weigh more than an uncharged one. I have not calculated whether the effect should be measurable or not.

In a work extending over many years I have naturally incurred indebtedness on many sides ; I have more especially to express my gratitude to my present colleague, former assistant and sometime pupil, Professor S. M. Kintner, without whose invaluable constructive and experimental ability I should have been able to accomplish but a small portion of the needed experimental work. I have also to thank two other of my colleagues, Dr. Brashear and Professor Frost, for their kind help in many ways. To Helen M. Fessenden also I am indebted for much of the work done in reducing observations and in the preparation of tables.

APPENDIX A.

INVESTIGATION OF THE RELATION BETWEEN μ AND H.

It is evidently desirable that the correctness of this relation should be established with some degree of precision.

It was first put forth by Kennelly in the *Trans. Am. Soc. Inst. E. E.*, Oct. 27, 1891, and has been confirmed by Steinmetz, in a most elaborate and valuable paper on magnetism.[1]

It has also been verified by the author, in a research conducted expressly for that purpose, the method and results of which are given below.

Frölich's formula is $1/\mu = c + dH.$

Kennelly's formula is $\{1/(\mu - 1)\} = a + bH = \nu_i.$

The difference between the two is that Kennelly considers, as mentioned above, the induction contributed by the iron as separate and distinct from that contributed by the ether, in the same manner as, in Gladstone's law, we consider the refraction contributed by the matter as distinct from that contributed by the ether. There are thus two magnetic circuits in parallel; one, the intrinsic circuit, carrying a flux numerically equal to $B - H$, the other the extrinsic circuit, carrying a flux numerically equal to H. As they have both the same difference of magnetic potential across them, the magnetic resistivity, or reluctivity, is, in the two cases, numerically $H(B - H)$ and H/H.

Kennelly's law means that this intrinsic reluctivity, ν_i is given by the formula $a + bH$, or in other words, that this intrinsic reluctivity varies as the space rate of drop of magnetic potential H varies.

---

[1] Trans. Am. Inst. E. E., Sept. 27, 1892.

Frölich's formula fails for high magnetizations. Kennelly's does not. Kennelly's formula gives a constantly increasing intensity of flux, with increasing H. Frölich's formula gives the intensity as reaching a limit. This former appears from Ewing's experiments by the isthmus method to be the correct statement.

As will be seen from the papers of Kennelly and Steinmetz, the agreement of the formula with theory is fairly close in all cases examined. It had however been stated [1] that it was a purely empirical formula and was not the expression of any physical law. The writer, therefore, made a set of tests with a view to obtaining results of as high a degree of accuracy as might be possible.

In taking curves of magnetism, the following points are important:

1. Long wires cannot be used, as the leakage varies with the reluctivity, and the area of leakage surface is relatively large compared with the cross section. At low values of μ the results may be many per cent. out.

2. Rings wound from wire cannot be used, as the magnetic flux has to jump from one wire to another through an air gap, and thus an additional reluctance is introduced, and moreover the inductance is not uniform, and the true shape of the curve is greatly masked. Steinmitz has shown[2] that when the flux has to pass in this way the variation from the mean induction may be so great as to increase the mean hysteresis 70 per cent. in sheet iron, and no less than 900 per cent. in iron wire, above the hysteresis corresponding to the mean induction.

3. Rings punched out of sheet iron are the only ones permissible, as it is not possible to weld a solid iron ring and keep its constitution and fiber uniform. Moreover, magnetic creeping is worse in solid rings.

4. The outside diameter of the ring must not be more than ten per cent., preferably not more than five per cent., greater than the interior diameter. In a ring of interior diameter 6 inches, external diameter 10 inches, which the writer once saw used for the determination of a permeability curve, it is evident that when H was 3 on the outside circumference of the ring it was not less than 5 on the inside circumference. Consequently, when the outside portion of the ring was on the downward part of the reluctivity curve, the inside part was on the upward side, and the curve obtained from such a ring must be entirely misleading, the features of the curve being run together, and the true reluctivity being at one point much lower than it would appear to be from the test.

This fact, ~~that the diameter~~ of the ring may affect the results by five per cent. or more, has been pointed out previously by Kennelly.

5. Rheostats with sliding contacts cannot be used for this work, as they give very insidiously distorted curves. This is due to the fact that if we are at P, Fig. 6, and the resistance in the circuit is, say, 200 ohms, and the next resistance 400, then on moving the contact the resistance does not change from 200 to 400, but from 200 to, say, 800; and from 800 to 400, due to the fact that when moving from one section to another the contact is not very good, and though of course not necessarily broken with a properly designed rheostat, yet the contact is worse when moving from one section to another than when at rest.

The result is, curves of the shape shown in Fig. 6 in the dotted line, where E, after sinking down lower than it should, comes back to a lower point than it would otherwise have fallen to. The result is a flattening, or even a concaving of that part of the curve.

A similar thing occurs on the up curve, and on the plain induction curve. The distortion is small, but since the curve is very steep, it makes a large percentage error in calculating the ferric reluctivity.

---

[1] Elec. World, June 9, 1894.

[2] On Hysteresis, Trans. Am. Inst. E. E., 1892, pp. 699 and 701.

6. All throws of the galvamometer must be of the same magnitude. This because the law of the galvanometer cannot be depended upon, the theoretical laws being only approximately followed and not closely enough where an accuracy of $\frac{1}{10}$ per cent. is required. A much more important reason is that the amount of creeping seems to be approximately proportional, according to some experiments of the writer's students, to the change of induction. It is therefore evident that, by changing B by a constant amount each time and taking approximately the same time for each throw, the results will probably be more reliable than when working more or less at haphazard.

7. If we change H to H' and then B to B' the state reached at B' will not be stable, even after creeping has stopped.

For if we wind on the iron ring a few turns of wire through which a very feeble current may be sent, which we call dH, then, on changing H from say 3 to 4, and then observing the value of B, if we next throw on dH so that its gilbertance is in the opposite direction to that of the main coil, we shall find that B is diminished by, say 50 lines. If, however, the direction of dH is the same as that of H, then B will be increased by, say 5 lines only. This would be bad enough, but the matter is still further complicated by the fact that if, after changing H from 3 to 4 and then throwing dH on in the same direction as F, giving an increase of from B to $B' + 5$, we then reverse dH, B' does not fall, as might be expected from $B' + 5$ to $B' - 50$, but only to $B' - 5$. What the ultimate value of B' is, then, when $H = 4$ and dH has been removed, depends upon which direction dH was first applied.

It is, therefore, impossible to get accurate magnetic curves with a dynamo current, as the minute fluctuations affect the result, and may not affect it twice in the same way.

This effect was investigated by one of the writer's students, Mr. Davis,[1] in 1896–7. The amount of the effect seems to depend upon the permeability. For this reason the step by step method was used in preference to that of reversals.

An additional reason for using a battery current is that small changes of H due to small pulsations in the current, as the brushes pass over the commutator segments, seem to change the rate of creeping, and also the amount.

The existence of the above phenomena is an additional reason against using a rheostat with sliding contact.

8 The time constant of the circuit must be small as compared with that of the galvanometer, as otherwise the system will begin to move before all the discharge has passed.

Sometimes the time constant is larger than would be expected without calculation.[2]

In the test about to be described the above effects were taken into account, as well as others, better known, due to thermo-electric currents, leakage, heating of resistance, and lack of uniform temperature.

The rings were of the best kind of transformer iron, cut to size on a tinner's machine, not punched, and carefully annealed. They were kindly furnished by the Westinghouse Co. In other samples, where the method of annealing was such that gas or carbon might be absorbed in the annealing, I have found that good results were obtained by enclosing the rings, embedded in sulphur-free iron peroxide, in a cast-iron box, with sides one inch thick. Some of the peroxide will be reduced, and it can be seen whether the gas has got to the plates or not by the difference in color of the oxide.

---

[1] Thesis, "On the Closed Magnetic Circuit in Telephones."

[2] In the case of the field rings for the Niagara generators, the time constant was measured in minutes, and Mr. Scott and the writer had, therefore, to devise a method in which the time rate of change of induction was kept constant, and the time for the complete discharge was the thing noted.

Each individual ring was examined to see if the coating of oxide was abnormally thick or held much magnetic oxide. If so, it was rejected. So far, the writer has been unable to find any way of getting at the true cross-section of the iron without destroying the rings in cracking the scale off, but for the present purpose that was unnecessary, as relative values only were required and a mistake as to the cross-section would affect all constants in the same ratio. The accepted rings were measured across three diameters, inside and outside, on the bed of a comparator.

This may seem an unnecessary refinement, but it was the shortest way, and it was thought best to know the mean circumference with some degree of accuracy.

The rings were then shellaced, dried, built up into one thick ring, 10 inches inside diameter, 12 inches outside diameter, and 1 inch thick. This was then carefully insulated with varnished paper, and both primary and secondary wound symmetrically, *i. e.*, the wires spired evenly round the whole of the circumference so as to prevent uneven distribution of magnetic leakage. Then the whole was shellaced and dried.

The galvanometer was astatic, of a well-known type, with four bell-shaped magnets in the system, being rendered more or less sensitive by screwing up a ring on one of the magnets.[1]

It was enclosed in a cylinder of soft cast iron, with top, sides and bottom of a thickness of 6 inches. The mirror was true, and the cover glass of the galvanometer was a parallel plane, ground by the writer at Mr. Brashear's shop.

The scale had been tested by the Société Genevoise comparator mentioned above. The telescope was of the usual type, and special arrangements were made to secure good lighting of it.

The suspension was a quartz fiber, 6 inches long. The period of the system was 20 seconds. reached by laying a piece of a magnetized file inside the iron case between the bottom of the case and the bottom of the galvanometer and moving it about until the desired sensibility and position were obtained.

The variable resistance was made after the fashion of a Varley rheostat, two wires wound in grooves on two pairs of wooden cylinders 1 foot in diameter. But along one cylinder of each pair was laid a metallic strip with holes drilled in it. In these holes small copper plugs could be screwed, in such fashion that where there were no copper plugs the wire passed over the strip, but where a plug was inserted the wire rested on it and so made contact with the strip. In turning the cylinder the wire was wound up until it reached the groove where there was a copper plug, when the contact was made and the resistance of the circuit decreased by the amount desired. In hysteresis tests, one pair is used for the up curve and one for the down.

A preliminary run was made between the values of H desired, with the plugs arranged by guess. From this the position in which the plugs must be placed to give equal deflections on the galvanometer were calculated, and the plugs so placed. The wires were German silver, of ample carrying capacity, and the heating very light owing to this and to the large surface exposed to radiation. The ends of the pins were grooved so that the contact should be good and sharp.

The iron was demagnetized by careful reversal, in diminishing cycles, combined with tapping, the ring being hung so that the plane of the ring was perpendicular to the direction of the earth's total magnetic field. The ring at first had an alternating current generated in it, with a view to shaking out the magnetism,[2] but this was afterward abandoned, as it was thought that it might introduce errors by possibly leaving some circular magnetization. The current was measured by a Weston milliammeter which could be read to $\frac{1}{15}$ per cent. of the maximum magnetizing current used, *i. e.*, about 1.45 ampères. The proportionality of this scale had been corrected for a particular position and orientation in the laboratory, by an accurately calibrated slide wire bridge, almost free from thermo-electric effects. Its absolute constant was determined by a Kelvin centi-ampère balance, whose constant had been frequently determined, but not much care was taken with this, as only relative values were desired and any great accuracy as regards this point would indeed have been absurd, in view of the fact that the true cross-section of the iron could not be accurately known within several per cent., so that absolute values were unobtainable.

---

[1] Willyoung, Elect. Eng., N. Y., Nov. 16, 1892.

[2] Ewing, Mag. Ind. in Iron, p. 318.

The ring was suspended in slings in a wooden frame, to prevent vibration and allow free radiation, and placed so as to be free from all outside magnetic influences. The temperature of the galvanometer, ring and ammeter were known. The measurements were made in the spring of the year, and during the hour which each set took, approximately, the temperature of the basement room did not change, as a rule, more than two degrees, whilst the galvanometer temperature change was so slight (in its double covering), that it never reached $\frac{1}{10}$ degree, which was as small a change as the thermometers used could show. The wire used on the iron ring, consisting of flat strands of fine wire for both primary and secondary was of ample cross-section, for the double purpose of keeping the heating negligible and of having that part of the resistance outside the galvanometer box as small as possible, so that changes in the room temperature would have but a small effect on the total resistance of the secondary circuit.

In making the test, approximately the same time elapsed between each throw. Each throw was about 15 inches, and as the inches were divided into tenths and each tenth could be read to the decimal part, the accuracy of the reading must have been approximately $\frac{1}{10}$ per cent. The constant of the galvanometer was determined, roughly, as accuracy was not necessary here, by a mutual induction coil, wound on glass. By means of this coil and the Weston milliameter mentioned above, the relation between quantity and throw was very accurately determined, especially for that part of the scale which the readings obtained in the experiment proper fell upon.

The third and best curve obtained is given in Fig. 8. The dots representing the observations are covered by the line, the greatest deviation from the line being almost exactly $\frac{1}{5}$ per cent. of the maximum ordinate, when all corrections were made. Below $H = 2\frac{1}{2}$ the line begins to deviate from the formula, and below $H = \frac{1}{2}$ the agreement is not even approximate.

As a further example of the accuracy obtainable by this method it may be said that a set of six hysteresis curves was taken, in the following year, by Mr. Kennelly and the writer in conjunction, with the same apparatus (with the exception of the resistance cylinders, for which were substituted separate coils hanging in mercury cups and suspended in mineral oil, being short circuited or cut in circuit, as it was desired to vary the current) and, with much more unfavorable temperature conditions, the worst hysteresis curve obtained closed up to within $\frac{1}{4}$ of one per cent. of the difference of B between $H = +$ max. and $H = -$ max. the best curve being out $\frac{1}{10}$ per cent., and the error of the other curves lying between these values. Without the above mentioned precautions the writer has never been able to *depend* upon hysteresis curves closing up within two per cent.

The curvature at the commencement is always found. It becomes much more sharp when the difference between the inside and outside diameters of the ring is taken into account.

If not, the error may amount to several per cent., and, in fact, for accurate results this must always be allowed for.

In the curve given, Fig. 7, a curve was drawn first from the corrected observations.

Then from the values of a and b obtained from this first curve, an allowance was made for the error caused by the mean permeability not being the permeability of the mean length. From this the correct curve was drawn, it not being necessary to make a second approximation, as this would alter the values by less than the experimental probable error.

The elbow may be removed, as shown in the following curves, Fig. 5, taken from the observations of Gerosa and Finzi,[1] which show that when the iron is shaken up the elbow disappears, but that a and b are not greatly changed.

It is thought that the above experiment, made with a view to testing the matter as accurately as might be, proves rather definitely that we have here no rough empirical formula, but the expression of a physical fact; *i. e.*, that the intrinsic reluctivity is a linear function of the drop of magnetic potential per linear cm. This may be considered the touchstone of the electrical theories, and as proving without doubt that κ is a density and μ the reciprocal of an elasticity.

The following formulæ are given, as being of use in electromagnetic design:

$$(1, 2, 3)\quad \nu_i = a\Sigma_0^{\alpha} bB = a + bH = a/(1 - bB).$$

$$(4)\quad B = H/(a + bH). \qquad (5)\quad H = aB/(1 - bB).$$

$$(6)\quad .4\pi nI = P\left[\frac{aL'c}{R - bPc} + \frac{a'L'c'}{R' - b'Pc'} + \cdots\right].$$

$$(7)\quad \frac{dB}{dH} = a/\nu_i^2 = aB^2/H^2. \qquad (8)\quad \frac{b\eta}{a} = .007 \text{ approx.}$$

Where B has been, for practical purposes, taken as equal to B_i, in nos. 4, 5, 6 and 7. I represents current, n = number of turns, P = total magnetic lines, B = lines per sq. cm., H = difference of magnetic potential per cm., $\nu_i = 1/(\mu - 1)$, L = length of part of magnetic circuit considered, R = cross-section of same, c = leakage coefficient of same, η = hysteresis loss per sq. cm. per cycle.

APPENDIX B.

RÉSUMÉ OF PREVIOUS WORK.

It was pointed out, in another portion of this paper, that given a knowledge of the nature of the electric and magnetic quantities, there is no difficulty in inventing suitable theories in regard to configuration, the number of such theories being chiefly a matter of permutations and combinations, but that each theory of the configuration virtually implies a theory of the constitution of matter.

In the present appendix it is not my intention to give an account of all the work done during the search for a solution of the problem with which this paper is concerned, for a record of this work, which has occupied a considerable portion of my leisure during a period of ten years, would be very voluminous. I shall deal only with that part of it which tends in some way to throw light upon the nature of the atom, and with that but very briefly. The results will be given in logical order and not in the order in which they were obtained.

PROPERTIES OF THE ATOM.

Various estimates have been made of the size of the atoms. There is, however, this question to be first answered: "Have the atoms definite sizes?"

If we take 45 c. c. of potassium and 18 c. c. of solid chlorine they can unite chemically to form a quantity of KCl occupying a space of but 37 c. c. One natural explanation of this fact that has been suggested, is that even in a solid the atoms do not lie closely adjacent to each other and hence there is abundant space for contraction even after the addition of other atoms. But this appears to be negatived by a number of facts. In the first place it is difficult to see how fusion can mean anything else than that the atoms have been driven so far apart by reason of the increase of kinetic energy that they can slide over one another and take each other's places. Hence in a solid it seems reasonable to suppose that they are so close together that this sliding over is impossible. Secondly, from Van der Waal's equation we may make an estimate of the volume of the atoms compared with that of the space occupied by them at the critical volume, and, hence, if we further deduce what this ratio must be for the solid state we see that the atoms must be nearly touching each other in this latter condition. Again, the fact that the electric conductivity and some other properties vary directly with the temperature as the temperature is lowered whilst these same properties are much altered by change of density, renders it hard to believe that there is any abrupt change in the volume of solids as we approach the absolute zero, which would apparently be necessitated by this explanation.

---

[1] See Ewing, Mag. Ind. in Iron, p. 319.

A second theory is that of Shröder, who assumes that the atom may have two or more different volumes, the volume of the atom depending upon the nature of the chemical combination.

It has been shown by the writer[1] that neither of these suppositions is necessary. If we take 100 balls of pitch,[2] each having a diameter equal to that of a cube containing .45 c. c. and stack them in rectangular array, as shown in Fig. 1, *a*, they will occupy a space of 45 c. c. We may assume the rectangular order as that naturally taken by bodies attracting each other and at the same time repelled by their mutual collisions. If the repelling force be increased by further heating, they separate further and can now slide over one another. If the kinetic energy be lessened, or some additional attractive force brought into play, the atoms come closer together and the substance is no longer isotropic but tends more to be crystalline.

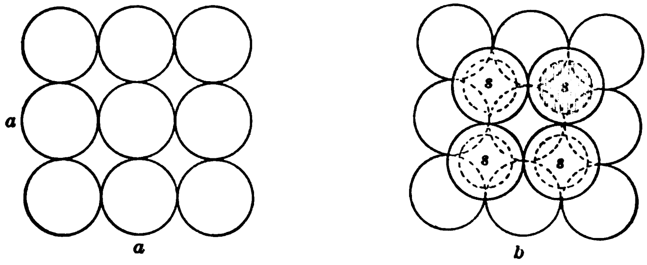


Fig. 1.

Take now 100 other balls, each having a diameter equal to the side of a cube whose volume is .18 c. c. Stack them in rectangular order and they will occupy a space of 18 c. c.

Next arrange alternate layers of the large and small balls, as shown in Fig. 1, *b*, where the attraction of the chlorine atoms is supposed to have pulled the potassium atoms over, as shown. On measuring the volume now occupied by both sets of balls it will be found to be nearly 37 c. c.

---

[1] Elect. World, Aug. 22, 1891. Science, March 3, 1893.

[2] Best made by pouring a slab of pitch, cutting up to *weight* and rolling between boards covered with pigment. Or, if a squirting machine is available, by cutting up squirted rod in proper lengths and rolling as before.

This simple experiment may not be considered convincing. But when we find, on applying the simple formula thence derived,[1] that we obtain not only the correct molecular volumes of KCl, NaCl, KOH, NaOH, Cæs. Cl, Rub. Cl, and other salts, and when in addition we find that in most cases so far tested the experiment also gives the correct crystalline angles,[2] we are to a certain extent justified in holding that

A. The space occupied by the atoms of a metal to the exclusion of other atoms is of an approximately spherical shape at ordinary temperatures.

B. The atoms of a metal, the metal being in a solid state, are not widely separated, but the shortest distance between the surfaces of two adjacent atoms is of the order of one per cent. of the diameter of the atom itself, when at temperatures not near the fusion point.

Metalloids, as we know from their low specific heats, form molecular groups in the solid state. Hence we may have more than one melting point, first when molecule can slide over molecule, and second when atom can slide over atom. If the atom or molecule be very irregular in shape, a separation sufficient to permit of this sliding may so weaken the cohesive force that it is overbalanced by the kinetic repulsion and the substance gasifies without melting.

Since also we find that it is possible to predict the crystalline shape of but few of the metalloids by means of the method mentioned above, we are led to consider the following statements as probable. For definition of metalloids see Fig. 2.

C. Metalloids differ from metals in that their atoms have not an approximately spherical shape.

D. The melting point and expansion of the elements is a function of the shape of the atoms.

Cauchy, in 1835 made the first estimate of the size of the atom, and in 1870, Kelvin by other methods arrived at the conclusion that the mean distance between the centers of contiguous molecules was less than 10^{-8} cm., and more than 5.10^{-10} cm.

Recent determinations by different methods agree quite closely, and it would seem as if the mean of the results cannot be far from the truth.

Given by.[3]	**Value.**
Nernst, Theoretical Chemistry	2.58×10^{-8}
Nernst, " "	2.69×10^{-8}
Risteen, Molecules and Mol. Theory	3.8×10^{-8}
J. J. Thomson, Recent Papers	3.34×10^{-8}
R. A. Fessenden, unpublished	2.79×10^{-8}

Taking into consideration the direction of the probable error of these determinations we obtain:

E. The diameter of the mercury atom is $2.75\ (\pm 0.2) 10^{-8}$ *cm.*

F. The ionic charge is $4\ (\pm 1) 10^{-10}$ *E. S. units.*

This ionic charge, "the natural unit of electricity," as Lodge aptly calls it (first calculated by Everett, and later, independently by Lodge) has long been considered as the cause of chemical action. This theory has been worked out in considerable detail by

---

[1] Science, March 3, 1893.

[2] Molecular Physics, Frank. Inst., September, 1896.

[3] Nernst, from Clausius-Mossotti theory of dielectrics and from kinetic theory of gases; Risteen from surface tension of mercury by Kelvin's method; J. J. Thomson from ionic equivalent of gases; Fessenden from surface tension of mercury by Ostwald's method. All reduced to mercury atom.

Davy, Berzelius, Helmholtz, Arrhenius, Clausius, Ostwald, Nernst and others, with the result that we no longer consider "chemical force" as a separate force but merely as a particular manifestation of electric force.

The writer was able in 1890 to make a still further extension of the field of electric action by showing that there is no necessity for assuming a separate "cohesive force," but that the ionic charges fulfilled yet another distinct function, in that they are capable of accounting for the tensile strength, rigidity, Young's modulus and other mechanical properties of bodies. The field of physics, by the removal of these two forces, has thus been reduced to the consideration of the forces connected with electricity, magnetism, gravitation and inertia.

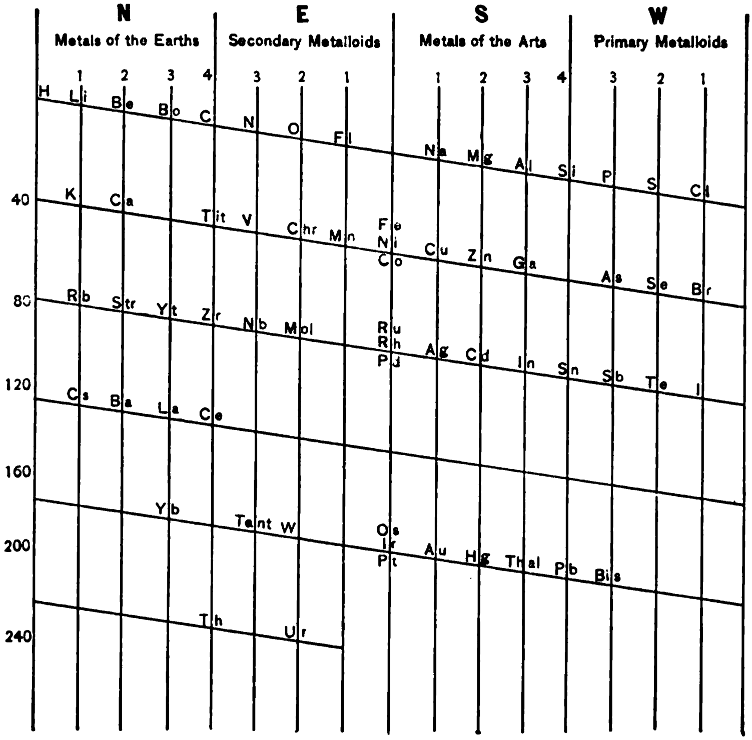

N
Metals of the Earths
E
Secondary Metalloids
S
Metals of the Arts
W
Primary Metalloids
1 2 3 4 3 2 1 1 2 3 4 3 2 1
40
80
120
160
200
240
H Li Be Bo C N O Fl Na Mg Al Si P S Cl
K Ca Tit V Chr Mn Fe Ni Co Cu Zn Ga As Se Br
Rb Str Yt Zr Nb Mol Ru Rh Pd Ag Cd In Sn Sb Te I
Cs Ba La Ce
Yb Ta nt W Os Ir Pt Au Hg Thal Pb Bis
Th Ur

Fig. 2.

According to this theory,[1] the normal atom, in the neutral state, is really a doublet, *i. e.*, has two equal charges, one positive and one negative. Two such atoms when at a distance will exhibit no attraction for each other, but on being brought near one another will be attracted. Attraction under these circumstances we take to be cohesion. "If however, any third substance" (or as was pointed out later, any other molecular form of the same substance) "connects the outside parts of the atoms and so enables these parts to neutralize each other, we have chemical combination, and the two parts when separated show opposite charges, as at C."[1]

---

[1] Not published till July, 1891, as the then novel idea of the atoms of metals having charges was considered by the editors to whom it was submitted as incompatible with the law that all charges must reside on the surface of conductors.

According to the electrostatic theory of cohesion, therefore,

G. Chemical action and cohesion are both manifestations of the same force, i. e., electrostatic attraction, under different conditions.

The resultant product of a cohesion effect is generally isotropic, whilst that of a chemical action (owing to the presence of atoms of different sizes or valencies) is crystalline, *i. e.*, "the atoms in one case may be grouped in any direction, and in the other they are only grouped about certain axes or planes."[2]

Taking the ionic charge as 4.10^{-10} E.S. units and the diameter of the silver atom as $2.42\ 10^{-8}$ cm., we find that the total quantity of electricity on a sheet of silver one atom thick and one cm. square is $6.8\ .10^5$ E.S. units. If for a first approximation we suppose the flux to be uniformly distributed we find for the attraction between two such sheets 3.4×10^7 dynes.

Considering a silver wire one mm. thick as made up of discs one mm. in diameter, we find, on this theory, that the attraction between two such discs is 270 kilogrammes.[3]

From Wertheim's experiments the tensile strength of a silver wire one mm. in diameter is 29 kilogrammes.

We have taken the flux density as uniform. We know that this cannot be true or the resistance to lateral shear would be very slight, in fact the substance would be a fluid. The amount of the attraction will depend upon the distribution of the charges, but we will find, on examination, that if we reject the idea of indefinitely large flux densities at any point, any conceivable distribution will give results of the same order of magnitude as those given above.

In this calculation we have neglected the kinetic repulsion. Our justification for this is that since the kinetic repulsion varies roughly as the cube of the distance between the surfaces of the atom, and the attraction varies as some power less than the square of that distance, the force required to stretch, shear or pull apart a body will, to a first approximation, be determined by the attractive force alone.

If the theory be correct and the disposition of the charges does not vary greatly for atoms of different elements, the mechanical properties should be functions of the size of the atoms, since the ionic charge is the same for all atoms. The following formulæ and tables show that such is the case :

A. Rigidity $= 28 \times 10^{12} \div (\text{atom. vol.})^2$

B. Young's Modulus $= 78 \times 10^8 \div (\text{atom. vol.})^2$

C. Velocity of Sound $= \{78 \times 10^{12} \div (\text{atom. weight} \times \text{atom. vol.})\}^{\frac{1}{2}}$

D. Tensile Strength $=$ Absolute Temp. of Melt. pt. $\div (1.92 \times \text{atom. vol.}^{\frac{1}{3}})$

The last is for wires 1 mm. in diameter.

The elasticity of such substances as India-rubber, gelatine and ivory was investigated. It was found that the abnormally high value of Poisson's ratio for such substances was due to the fact that their elasticity, as measured in the ordinary way, bears no direct relation to the true elasticity of the material, but is a matter of configuration. It was shown[4] that such substances consist of two or more compounds, interpenetrating but not combining with one another. It was shown that the heating on stretching, the cooling on relaxing and the shortening when stretched and heated, followed as a natural consequence of the configuration. The different compounds were differentiated under the microscope and, in the case of India-rubber, by following Faraday's method, isolated chemically. The shape of the elasticity curve was predicted and the prediction was verified by the writer and by Professor Thurston.[1] Substances were made which had the shape of the elasticity curve, the property of polarizing light on being stretched, and the value of Poisson's ratio the same as rubber, but which on simple pressure could be differentiated into two substances, each of which had the usual form of elastic curve, now heated instead of cooling, on compression, and now had a value of Poisson's ratio nearly one-half of what it was previously. The behavior of these abnormal substances was thus shown not to conflict at any point with that called for by the electrostatic theory of cohesion.

---

[1] Science, July 22, 1892. J. J. Thomson showed later that the proximity of a dielectric of greater capacity would have the same effect.

[2] Molecular Physics, Franklin Inst., Sept., 1896.

[3] Given as 45 kilogrammes in *Electrical World*, August 22, 1891, owing to a slightly different value having been taken for the size of the atom.

[4] Mol. Phys., Frank. Inst., September, 1896.

TABLE I.

Metal.	Atom. Vol.	Rigid. $\div 10^9$	Calc.	Young. M. $\div 10^9$	Calc.	Tens. str.	Ca
Iron.	7.1	750	550	2000	1560	65	74
Copper.	7.1	430	550	1220	1560	41	48
Platinum.	9.1					35	48
Zinc.	9.2	350	340	930	920	15.7	16
Silver.	10.2	280	270	740	750	29.6	29
Gold.	10.2	270	270	760	750	28.5	29
Aluminum.	10.4	250	260	680	690	18.	18
Cadmium.	13.		170	480	465		
Magnesium.[2]	14.	150	143	390	395	10.4	9
Tin.	16.3	136	100	420	295	3.4	5
Lead.	18.1	84	83	190	235	2.36	4

It was found that there existed a relation between the velocity of sound and the electric conductivity of the pure metals as follows :

H. If we take wires of two pure metals, having an equal number of molecules in the cross section, then the resistances of the wires will be proportional to the times taken by a sound wave to traverse them.

The supposition is here made that whilst, as regards specific heat, the pure metals behave as if there were only one atom in the molecule, as regards electricity they behave as having the number of atoms per molecule numerically equal to the valence. The grounds for this assumption may be questioned, the reasons for making it are given in another paper.[3]

The formula for the electric resistivity at 0° C. of any metal is thus :

$$\text{Resistivity} = 14.04 \times \text{atom. vol.} \times \sqrt{\text{atom. weight}} \times \text{valency}.$$

Table II. on opposite page shows the observed and calculated resistivities.

The tests on Zn, Sn and Pb were made on pressed wires.

It should be pointed out that the conductivities of two substances, which, as determined at the time when this relation was first published,[4] were too low, have since been found, by later experimenters working with pure materials, to be in agreement with the formula. I refer to aluminium and magnesium, whose resistivities were given as 2881 and 4162 respectively, but the former of which has been found by Richards and Thomson [1] to have the restivity 2370 for the pure material, and magnesium, which has been shown by Thurston [2] to have a resistivity of 3835. In the case of zinc, tin and lead it will be noted that through the absolute values are low, the ratios are correct. It is barely possible that the fact that these three materials were all tested in the form of squirted wires may have something to do with the discrepancy.

---

[1] Science, May 1898.

[2] The tensile strength of magnesium was predicted, Science, July 22, 1892, and Thurston's tests, London Elect., June 5, 1896, showed it to agree with theory.

[3] Conduction and Insulation, Amer. Inst. E. E., March, 1898. [4] Science, July 22, 1892.

TABLE II.

Metal.	Atom. vol.	Atom.wt.$^{1/2}$	Valency.	R. calc.	R. obs.	Observer.
Cu.	7.1	7.94	2	1583	1580	Mathiessen.
Ag.	10.2	10.39	1	1488	1488	"
Au.	10.2	14.05	1	2012	2036	"
Al.	10.5	5.2	3	2300	2370	Rich. & Thomp.
Mg.	14.	4.9	4	3852	3835	Thurston.
Zn.	9.4	8.09	4	4270	5566	Mathiessen.
Cad.	13.	10.58	4	7722	6720	Benoit.
Sn.	16.2	10.86	4	9880	13070	Mathiessen.
Pb.	18.2	14.38	4	14695	19420	"

This formula, whilst in some cases not entirely satisfactory (though in view of the fact that already in two cases the fault has been found to lie with the physical measurement and not with the formula, it may happen that the other exceptions may also fall into line) is of interest as linking together the phenomena of the conduction of sound, heat and electricity in wires. I have suggested elsewhere that the velocity with which heat and electricity travel in a wire may be that of sound. In the case of heat, the experiment might possibly be made, in spite of the very rapid logarithmic tailing down, due to the wire's heat capacity (hence similar to the case of an electric cable), by a thermocouple at one end of a short piece of gold or platinum, heated very suddenly at the other end by electric means. But in the case of the electric current, as Maxwell and Heavyside have pointed out, we know absolutely nothing of the velocity with which electricity travels in the wire. "It may be an inch an hour or it may be immensely great." [3]

The theory was applied to fluids. It was shown that whether a body is solid or fluid or a gas or vapor depends upon whether the fraction

$$\frac{\text{Cohesive force of atoms for one another} + \text{external force}}{\text{Kinetic repulsion} + \text{cohesive attraction for other atoms}}$$

is greater or less than unity. From this equation of state it will be seen that we can produce dissociation in two ways: by decreasing the numerator or increasing the denominator. If we increase the first term of the denominator we disassociate by heating, if we increase the second term by bringing the atoms, say of NaCl, in contact with other atoms, of say H_2O, we disassociate by solution. It is shown that this leads us to the idea of an "osmotic suction" instead of an "osmotic pressure," and that the change in the

[1] Journal Franklin Institute, March, 1897.

[2] London Elect., June 5, 1896.

[3] Heavyside, Papers, II., p. 394.

"volume force" thus brought about accounts for the heating on solution, and other difficulties of the "osmotic pressure" theory. Some applications were made to the theory of electrolytic conduction.

The theory was also applied to gases, and it was pointed out that the constant a in Van der Waal's equation

$$(p + a/v^2)(v - b) = RT$$

should be a function of the volume b. That this is true is shown by the following table, the values being taken from Ostwald's "Outlines of General Chemistry,"

Substance.	$a \times 10,000$	$b \times 10,000$.
Dyethylamine	355	58
Ethyl. Acet.	348	55
Ether	324	57
Benzine	(438)	51
Ethyl. Form.	304	48
Chloroform	287	44
Acetone	273	44
Methyl. Acet.	248	39
Alcohol	236	37
Ethyl. Chlor.	227	40
CS_2	219	33
SO_2	123	24
NO_2	(74)	19

and a modified form of the equation, *i. e.*,

$$(p + c/b^{4/3})(v - b) = RT$$

was suggested, c having the same value for all gases.

Although received with so little favor at first that it was over a year before publication could be obtained, the theory seems now to be on a better footing. The electrification of separate sheets of mica, etc., is in its favor, and Ostwald pointed out in a letter to the writer, September 16, 1891, that some experiment of his along a similar line were of interest in this connection.[1]

About a year and a half after its first publication, it was shown by Chattock (Phil. Mag., Dec., 1892) that the piezo-electric and thermo-electric properties of crystals, and the dielectric strength of gases, could be accounted for along these lines.

In the case of these phenomena, since we are dealing with chemical compounds, and not simple elements, there is nothing to show that the effect is not merely a mechanical-chemical one, or that there does not exist a force of cohesion *in addition* to the force exerted by the electric charges which we know to be active in chemical action. But Chattock was led, in addition, independently, it appears, to the same conclusion as the writer, *i. e.*, that cohesion as well as chemical action is to be accounted for by the ionic charges.

---

[1] "The electrostatic theory of cohesion is new to me, and interesting, in as much as, according to my experiments, the cleavage surfaces of almost all bodies appear to be electrically charged." A point which he raises, "For electrolytes there would be the question to answer, 'Why substances like alcohol, etc., show no ions?' whilst according to your theory, all elements have electric charges" is, I think, explained on taking into account the different ways in which the atoms are charged in chemical and cohesive effects.

MAGNETIC ROTATION OF LIGHT.

Both Kelvin and Maxwell, from a study of the phenomenon of the magnetic rotation of light, came to the conclusion that, to quote Maxwell, "at each point of the medium something exists of the nature of an angular velocity about an axis in the direction of the magnetic force."

I have never been able to persuade myself of the validity of the proof given, as, in spite of Maxwell's disclaimer, it seems to me that both proofs virtually imply a certain class of theories of light. If, indeed, it were proven that rotation is possible in vacuo, then we would be forced to conclude that some one of these light theories was true. But so far this has not been shown.

It is, I think, not difficult to see that the phenomenon may arise from a simple strain, and that an angular velocity is not necessitated. The rotation of a plane wave is reversed on reflection backwards through a suitably chosen medium, but the rotation of other forms of periodic motion may be doubled. Take a couple of wires of equal length, and string beads on them. Twist one into a right-handed spiral and the other into a left-handed spiral. Take any medium such that on stress it becomes a mass of right- or left-handed spirals, for example a lot of strands of wire rope, straightened and packed loosely side by side. In this condition, if equal forces be applied to right- and left-handed spirals, one will pass through the medium as fast as the other. Compress the bundle of strands, however. It will then turn to a mass of loosely packed right-handed spirals. Through this the right-handed spiral will now penetrate much faster than the left-handed. If, after passing through the medium, the wires strike a plate, and being bent back, are forced to reënter the medium and traverse it in the reverse direction, the right-handed spiral will still travel the faster, and its total gain will be twice that given by traversing the medium in the original direction.

So that the assumption that when we have a magnetic field we have an angular velocity about the axis of magnetic force really implies a theory of matter, and taken by itself, in the absence of any knowledge as to whether light can be rotated in a vacuum, is incapable of proving anything in regard to the nature of the magnetic field.

I have pointed out elsewhere[1] that "It might be supposed that the electro-magnetic rotation of light would give us a fourth equation. * * * Since the atoms carrying charges are vibrating in every direction, we have really an infinite number of electric currents, and it is obvious that when such a system is placed in a magnetic field the velocity will be greatest in a direction parallel to the current in the nearest part of the magnetizing coil, as if the motion be otherwise, work will be done in forcing the atoms to move in that direction. In either case we get only an equation between the electric and magnetic quantities which is implicitly contained in the first three equations."

This change in velocity when the ions are placed in a magnetic field has since been shown by Lorentz to be the cause of the Fiévez or Zeeman effect. It was, however, found that this ionic movement did not give results of the same order as those necessary for the explanation of magnetic rotation and the following theory (communicated to Professor Fitzgerald, December, 1898) was taken as being the most probable one.

In the electromagnetic wave the energy exists in two forms—electric and magnetic. Consider the electric displacement by itself., It produces, by itself, a magnetic force which is a maximum at the instant when the displacement is zero. So long as the energy is being transmitted without loss, the electric displacement and the electric force producing it are a maximum at the same instant. But suppose that there is absorption, then the voltage and displacement current will no longer be 90 degrees apart, but the angle will be less. The voltage may now be considered as split up into two components, one in phase with the displacement and one at 90 degrees from it, in point of time. Hence while, when there was no loss of energy, the plane of the electric displacement was merely rocked to and fro, there will now be a to and fro rocking due to the one component, together with a continuous twist due to the other component similarly with the magnetic component. In other words, whenever we have absorption, the light wave is rotatable when placed in a magnetic field. Some evidence in proof of this has already been obtained, and will be published later. Here it will be sufficient to point out that those substances which absorb most strongly, such as selenium, or copper oxide, give the greatest rotation, and that the rotation appears, in those cases so far examined, to be greatest at that point in the spectrum where the absorption is greatest.

---

[1] Electrical World, May 18, 1895.

THERMO-ELECTRICITY.

A number of experiments were made to see if there was any evidence that thermo-electricity depended upon the energy of combination of the materials of the circuit, analogous to the discovery of Exner that the voltage of cells could be calculated from the heats of combination of their elements. It was found, after examining the records of a number of thermopiles, that in every case the total amount of energy which had been obtained from them was of the same order as that which would have been furnished by the heat of combination of the materials forming the couples. Two holes were then bored in a slab of antimony, and the two ends of a copper rod of ½ inch diameter were driven in them. The whole was then fixed to a steam pipe in constant use so that one end was in contact with the pipe and the other in the free air. After a year the combination was examined. It was found that at one junction there was a considerable quantity of violet colored alloy of copper and antimony, whilst at the other there were only traces. As, however, one of the contacts had gone bad, at some time unknown, it was impossible to make a quantitative estimation, and the experiment will be repeated at a favorable opportunity. It is possible that thermo-electric effects are intimately connected with the phenomenon of molecular diffusion in solids, discovered by Roberts-Austen, and so strikingly illustrated by his admirable experiments.

CONFIGURATION OF ATOM.

Since every atom is charged and the charge has the quality M/T, *i. e.*, current of inertia, we thus for the first time have a definite proof of the vortex nature of the atom. A considerable number of other points were investigated, more or less fully, but will be discussed elsewhere.

APPENDIX C.

NOMENCLATURE, SYMBOLS AND UNITS.

Mental, as well as mechanical processes, are vastly facilitated by the presentation of the material to be worked up in a form adapted to the machinery which is to be employed upon it. When we have stated our data in terms of suitable units, we have accomplished a sort of preparatory rough planing and surfacing of our facts, and as a result we may then proceed to make our deductions with much greater ease and with greater certainty.

In some respects our present systems of units are very imperfect. Aside from the fact that the duodecimal system would have been much preferable to the decimal, a great opportunity was lost when the meter was taken as a decimal part of the earth's circumference instead of being made equal to twice the distance passed over in the first second, by a body falling from rest, at sea level, at Paris, and in vacuo. This would have made the unit force the weight of unit mass, instead of $\frac{1}{981}$ of the weight of unit mass. We do not care whether a block of metal of one kilogramme mass has its side such and such a fraction of the earth's circumference or not, but it would be very convenient to know that 200 units of energy would give it a velocity of 20 units of length per second, and that unit work would be done in raising it through unit distance. True, the relation would not have held exactly over the whole earth, but neither is the cubic centimeter of water exactly equal in mass to the gramme (it being about two-tenths of one per cent. less than a gramme at ordinary temperatures), and the practical advantages of this latter approximate relation have not, to my knowledge, been disputed.

If the unit length had been taken as 981 of our present centimeters, the weight of unit mass would have been equal to the unit force to within two-tenths of one per cent. (*i. e.*, to within the same degree of accuracy with which a cubic centimeter of water at ordinary temperatures represents a gramme of mass), over practically all those portions of the earth where the mechanic arts have attained their highest development. At the equator this practical unit of work would be slightly deficient in size, a circumstance which some slight acquaintance with the inhabitants of tropical countries leads me to believe would not be highly resented by them.

In our electric and magnetic systems especially, the state of affairs is very deplorable. In addition to the two necessary systems of units, *i. e.*, the electrostatic and the electromagnetic, we have a third one, which is absolutely unnecessary, *i. e.*, the so-called practical system.

To express the relations between the units of this system and of the electromagnetic system a large number of constants of various sizes are employed. This creates great consequent confusion and annoyance.

Again, as Heavyside has pointed out, the definition of unit quantity of electricity and unit quantity of magnetism by the equations

$$\frac{QQ'}{\kappa L^2} = \text{Force}, \qquad \frac{PP'}{\mu L^2} = \text{Force},$$

instead of by the equations

$$\frac{QQ'}{4\pi\kappa L^2} = \text{Force}, \qquad \frac{PP'}{4\pi\mu L^2} = \text{Force},$$

has the effect of introducing the very objectionable constant 4π into many of our most frequently used and most important formulæ.

Another point, which is equally important, is that the use of the present defining equations, given above, necessitates the employment of units of electric and magnetic flux in addition to the units of quantity of electricity and quantity of magnetism, the units of flux being of the same nature as the units of quantity, but being only $1/4\pi$ times the size of the former. This again gives rise to two distinct sets of coefficients, differing from each other in the same way.

On examination it will be found that the number of totally unnecessary electric and magnetic quantities thus introduced is not less than 46 in number, and, moreover, we must have the same number of additional unnecessary symbols.

To take an example, If we wish to know what electrostatic flux corresponds to 3 coulombs, we must

1. Divide by 10 to reduce to absolute electromagnetic units.
2. Multiply by 3.10^{10} to reduce to electrostatic units.

3. Divide by 4π to reduce to units of flux, the result being,

$$3 \text{ coulombs} = 23873 + 10^4 \text{ electrostatic lines.}$$

It seems[1] that there is a very strong feeling amongst the electricians and electrical engineers of this and other countries in favor of a return to absolute units and the abandonment of the practical system. Although some of the practical units have been legalized there is nothing to prevent our giving names to the absolute units and using them exclusively for practical work.

There remains, however, the 4π difficulty. Heavyside has proposed to get rid of this constant, or rather, to transfer the "eruption of 4πs" to a place where it will do no harm, by making the unit of current $1/\sqrt{4\pi}$, and the unit of voltage $\sqrt{4\pi}$ times as large as the present units.

But whilst this would undoubtedly have been the best way, if it could have been done in 1873, it is now not practicable. It necessitates changing the ampère, coulomb, volt, ohm, farad and henry. Most of these have been legalized, and a very large sum has been locked up in standards. Were any attempt made to change the present legal units, the government would probably, and justly, point out that it is only a few years since the present units were recommended for adoption, and request its petitioners to excuse the taking of any further action until some evidence was furnished that the proposed alterations were likely to meet with a permanent approval.

I have found, however, that by a great piece of good fortune there exists a way in which the 4π can be got rid of without disturbing any of the legal units. It consists in shifting the 4π from the unit of quantity into the coefficient μ.

From the equation

$$\text{curl}\,(QP) = \text{Work}/T$$

we see that, there being but one curl, we have but one 4π. If we keep our units of mass, length and time unchanged, we can get rid of the 4π in three ways: 1. By dividing up the 4π between Q and P, *i. e.*, making each $1/\sqrt{4\pi}$ times as large as before. This was suggested by Heavyside. 2. By putting the 4π over on the other side, *i. e.*, making the unit of work 4π times as large as before. This is impracticable. 3. By making one of the two quantities, Q or P, $1/4\pi$ times as large as before.

The third method is the practical one. We cannot change the coulomb, but we can take the unit quantity of magnetism as equal to the present line, and the unit of difference of magnetic potential as equal to the current turn, without creating any appreciable disturbance with the existing order of things. Neither of these quantities has been defined by law, and as a matter of fact the ampère turn is already used by designers as a practical unit of gilbertance.

We now have

$$dQ/dT = \text{voltance}$$
$$dP/dT = \text{gilbertance}$$

and have, therefore, now no use for the "flux" as distinct from the "quantity." We have, therefore, no use for the quantities, specific inductive capacity and permeability, *i. e.*, for the ratios

electric flux density ÷ volts per cm.
magnetic flux density ÷ gilberts per cm.

whereas we still have use for the ratios

quantity of electricity per sq. cm. ÷ volts per cm.
quantity of magnetism per sq. cm. ÷ gilberts per cm.

---

[1] See in this connection the following articles: Editorial, Electrical World, June 9, 1899. Blondel, Electrical World, July 29, 1899, and the writer, Electromagnetic Design. Franklin Inst., April, 1899.

No names have been given to these quantities, though the ratio

intrinsic quantity of magnetism per sq. cm. ÷ gilberts per cm.

has been termed the "susceptibility."

We might use the present terms, specific inductive capacity and permeability, to denote these latter ratios. But it is always objectionable to give an old name to a new quantity, and I therefore suggest that the ratios

$$Q \text{ per sq. cm.} \div F, \text{ and } P \text{ per sq. cm.} \div H$$

be termed the "capity" and "permity" respectively. The similarity of the names will indicate their relationship to the quantities specific inductive capacity and permeability, and we may retain the symbols κ and μ for the quantities capity and permity. As mentioned above, we have introduced the 4π into the constant u. The permity of a vacuum is then 4π, whereas the permeability for vacuo (or air. It should be vacuum, if v is given for vacuo and not for air) is now unity. The value of the electric constant remains the same as before, only, as we are now dealing with a different thing, *i. e.*, the ratio of quantity ÷ voltivity instead of flux ÷ voltivity, we now speak of the capity of a vacuum as being $\frac{1}{4}\pi$ where before we spoke of the specific inductive capacity of a vacuum as unity.

It will be seen that we have put the 4π where it will do no harm. The fact that we have now $\mu = 1846$ where before we had $\mu = 147$ is a matter of no consequence. No one uses published tables of permeability for designing, for no two samples of iron have the same value and as each sample comes into the factory it must have its constants determined just as if no other sample of iron had ever existed. As a matter of fact, most of our data with respect to slightly magnetic substances has been published in the shape of tables of susceptibility and not of permeability.

The same remarks apply to capacity. No one would think of using tables of capacities for making cables or condensers. Very little is known of the true capacity of substances, as this constant is affected to a very great extent by traces of moisture. I have myself been able to reduce Hopkinson's figures for oils very materially by using pure materials, carefully dried. As another example, the capacity of india rubber is given by Jenkin as 2.80, and by Salford and Halman as 3.7 whilst cable manufacturers, who use a special treatment to eliminate water, will furnish it as low as 2. No one would therefore think of using published tables for any practical or theoretical work of importance. In making cables or condensers a sample is always made up first and the capacity calculated from that.

I give below a table showing the relation between the present and suggested units and formulæ.

Present Units.	*Suggested Units.*
Coulomb	No change.
Ampère	"
Volt	"
Ohm	"
Farad	"
Erg. Joule, Watt	"
Specific Ind. Capacity.	No change, but we have now no use for the quantity specific inductive capacity, using capity instead.
Permeability.	$(4\pi)^2$ times as large. But having now no use for this quantity we use the permity, which is 4π for vacuo.
Quantity of Magnetism.	$\frac{1}{4}\pi$ times as large as before, *i. e.*, equals present line.
Difference of Magnetic Potent.	4π times as large as before.

Present Equations.	*Suggested Equations.*
$\frac{QQ'}{\kappa L^2}=\frac{PP'}{\mu L^2}=$ Force,	$\frac{QQ'}{4\pi\kappa L^2}=\frac{PP'}{4\pi\mu L^2}=$ Force.
$E=4\pi\frac{dP}{dT}$, $G=4\pi\frac{dQ}{dT}$,	$E=\frac{dP}{dT}$, $G=\frac{dQ}{dT}$,
$\frac{D^2}{8\pi\kappa}=\frac{B^2}{8\pi\mu}=$ Energy per c.c.	$\frac{D^2}{2\kappa}=\frac{B^2}{2\mu}=$ Energy per c.c.

I gave also a complete table of the quantities, symbols and units which would be necessary under the new system. It will be noted that 46 quantities, whose existence is now necessitated, owing to the present distinction between quantity and flux, have been dropped, as not required with the new system.

No importance is attached to the *names* suggested. They can, in most cases, be improved upon. Stress is however laid upon the symbols proposed, which it is thought will not readily be bettered. For vector quantities in the electric, magnetic and electromagnetic systems, block or script letters may be used, and ordinary type for scalers. Where, as in ælotropic substances, the capity, permity or conductivity has direction, cursives may be used. For the mechanical quantities, gravity and heat, italics are used. Vector quantities here may be either block or script. For radiation, German type is used.

The terminations have the following meanings.

a, Absolute.
ma, Absolute electromagnetic.
sta, Absolute electrostatic.
l, amount per cm.
r, amount per sq. cm.
v, amount per cubic cm.
t, amount per second.
rt, amount per sec. per sec.
st, amount per sec. per sec. per sec.

The syllable *al* denotes extrinsic quantity.

The syllable *id*, denotes intrinsic quatity.

It will be noted that all the regular uncials are free for use for other purposes, as also the letters d, δ, Δ and X, Y, Z.

The idea of using terminations in this manner is of course not new. Quite recently, since the above was written, the matter has been brought forward again in a very interesting paper by M. Hospitalier.

The changes suggested can be made without interfering with any of the legal units, without rendering useless any tables used in practical work, and will merely render it necessary to alter the values of the electric constants of the few substances whose capacities we know accurately. In fact, even this is not necessary, for since we have made no change in the unit of capacity, but have merely introduced a new quantity, the capity as being more convenient, those who choose may still employ the present term, specific inductive capacity, and use the present values.

It may be mentioned that if, at some future time, we shall revise our units, taking the mass of a cubic cm. of the ether as the unit of mass, and twice the distance passed over in the first second by a body falling, from rest, in vacuo, at Paris, at sea level, as our new meter, we may use a system of symbols and names naturally provided for us.

For, it will be noted that all physical quantities have their qualities in the form

$$\frac{M^{v}\,L^{z}}{T^{y}\,L^{x}}$$

where x, y and z may have the value 1, 2 or 3, and v the value 0 or 1. Consequently all physical quantities can be symbolized by using the one single letter *H* with different lengths of arms.

Thus, *F* represents electric current per sq. cm.

F' represents energy per cubic cm.

H represents stress per unit area.

For writing, we may use / for 1, (for 2, and) for 3.

Different fonts of type can be used to distinguish between electric, magnetic and other quantities. At present this suggestion would hardly meet with approval from the printers

Similarly, if we take *m* for M, *t* for T, *rt* for T^2, *st* for T^3, *a* for L, *e* for L^2, *i* or *y* for L^3, *u* for two L's in the same direction and *o* or *n* for zero for each possible physical quantity we get a corresponding word.

Thus, Elmore would represent electric current.

Mury would represent energy per c. c.

Marte would represent stress per unit area.

The matter is, however, for the present at least, one of curiosity rather than of practical importance.

ADDENDA.

The coefficient η, referred to several times, is the coefficient in Steinmetz's formula

$$\text{Hysteresis loss per c. c. per cycle} = \eta\, B^{1\cdot 6}.$$

Having these relations between H, B, μ, η and the elasticity, we are in a position to develop the quantitative mathematical side much further. For example, we may substitute in a number of terms in the Lagrangian function treatment of J. J. Thomson (Dynam. App. to Phys. and Chem.), and obtain a much more complete idea of the relation between elastic strain and magnetization.

The qualitative formula for temperature, given above, *i. e.*, energy per atom, is for monatomic molecules. It is better given as energy per molecule.

The statement that μ varies with H whilst κ does not vary with F is not to be understood as meaning that change of F makes *no* change of κ, but that it is not of the nature required to make κ a compliancy. It will, however, be seen that κ should vary inversely as F^2, for $M/L^3 = T^2/L^2 \times ML/T^2$. This phenomenon has as yet not been discovered.

ELECTRIC.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Magnetic Unit.*	*Size of Suggested Absolute Unit.*
Quantity	Quantity	Q	$Q^2/4\pi\kappa L^2 = \Phi$	Coulma	Same as present
Quantity per sq. cm.	Surface Density	D	$D \times \text{area} = Q$	Coulmar	"
Quantity per c. c.	Volume Density	C	$C \times \text{volume} = Q$	Coulmav	"
Quantity Coefficient	Capity	κ	$\kappa F = D$		" (Note)
Recip. of do.	Recusity	λ	$F/\lambda = D$		"
Quantity Factor	Capance	K	$EK = Q$	Faradma	"
Recip. of do.	Recusance	Λ	$E/\Lambda = Q$	Darafma	"
Potential Difference	Voltance	E	$EQ = \Psi$	Volma	"
Potential Difference per cm.	Voltivity	F	$E \div \text{length} = F$	Volmal	"
Intrinsic Quantity	Quantidity	Q_i	$Q_i + Q_e = Q$	Coulidma	"
Extrinsic Quantity	Quantality	Q_e	$Q_e = EK_e$	Coulalma	"
Intrinsic Quantity Coefficient	Capidity	κ_i	$\kappa_i + \kappa_e = \kappa$		"
Recip. of do.	Recusidity	λ_i	$\lambda_i = 1/\kappa_i$		"
Intrinsic Quantity Factor	Capidance	K_i	$K_i + K_e = K$	Farididma	"
Recip. of do.	Recusidance	Λ_i	$\Lambda_i = 1/K_i$	Darafidma	"
Extrinsic Quantity Coefficient	Capality	κ_e	κ_e is κ of ether		"
Recip. of do.	Recusality	λ_e	λ_e is λ of ether		"
Extrinsic Quantity Factor	Capalance	K_e	K_e is K of ether	Faradalma	"
Recip. of do.	Recusalance	Λ_e	Λ_e is Λ of ether	Darafalma	"
Increase per degree C	Temperature Coefficient	κ_B, λ_B			

NOTE.—No importance is attached to the names suggested, these being given merely for the purpose of illustrating the system. The terminations -a, -ma and -sta stand for the terms "absolute unit," "absolute electro-magnetic unit" and "absolute electrostatic unit" respectively. The terminations -l, -r, -v, -t, -rt, -st stand for quantity per cm., per sq. cm., per c. c., per second, per sec. per sec. and per sec. per sec. per sec. respectively. The syllable "id" is used for "intrinsic quantity" and the syllable "al" for "extrinsic quantity."

MAGNETIC.

Name.	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Magnetic Unit.*	*Size of Suggested Absolute Unit.*
Quantity	P	$P^2/4\pi\mu L^2 = \phi$	Gausma	$1/4\pi$ times present *i. e.*, equal to present magnetic line
Surface Density	B	$B \times \text{area} = P$	Gausmar	$1/4\pi$ times present
Volume Density	A	$A \times \text{volume} = P$	Gausmav	$1/4\pi$ times present
Permity	μ	$\mu H = B$		$(4\pi)^2$ times present (Note 2)
Reluctity	ν	$H/\nu = B$		$1/(4\pi)^2$ " "
Permance	M	$GM = P$	Rema	$(4\pi)^2$ " "
Reluctance	N	$G/N = P$	Oersma	$1/(4\pi)^2$ " "
Gilbertance	G	$GP = \psi$	Gilma	4π times present *i. e.*, equal to amma turn
Gilbertivity	H	$G \div \text{length} = H$	Gilmal	4π times present
Quantidity	P_i	$P_i + P_e = P$	Gausioma	$1/4\pi$ times present
Quantality	P_e	$P_e = GM_e$	Gausalma	$1/4\pi$ times present
Permidity	μ_i	$\mu_i + \mu_e = \mu$		$(4\pi)^2$ times present
Reluctidity	ν_i	$\nu_i = 1/\mu_i$		$1/(4\pi)^2$ " "
Permidance	M_i	$M_i + M_e = M$	Remida	$(4\pi)^2$ " "
Reluctidance	N_i	$N_i = 1/M_i$	Oersidma	$1/(4\pi)^2$ " "
Permality	μ_e	μ_e is μ of ether		$(4\pi)^2$ " "
Reluctality	ν_e	ν_e is ν of ether		$1/(4\pi)^2$ " "
Permalance	M_e	M_e is M of ether	Remalma	$(4\pi)^2$ " "
Reluctance	N_e	N_e is N of ether	Oersalma	$1/(4\pi)^2$ " "
Hysteresis Coefficient	η		Hergav.	
Temperature Coefficient	μ_E, ν_E			

2. In our present system, the permity of air is $1/4\pi$.

ELECTRO-MAGNETIC.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Mag. Unit.*	*Size of Suggested Absolute Unit.*
Quantity of Electricity per sec.	Current	I	$I = dQ/dT$	Amperma	Same as present.
Q'ntity of Electricity per sq. cm. per sec.	Current Density	J	$J = I \div \text{area}$	Ampermar	"
Quantity of Magnetism per sec.	Flow	U	$U = dP/dT$	Weberma	$1/4\pi$ times present size *i. e.*, equal to present line per sec.
Q'ntity of Magnetism per sq. cm. per sec.	Flow Density	V	$V = U \div \text{area}$	Webermar	$1/4\pi$ times present size.
Conductivity	Conductivity	σ	$F\sigma = J$		No change.
Recip. of do.	Resistivity	ρ	$F/\rho = J$		"
Conductance	Conductance	S	$ES = I$	Moma	"
Recip. of do.	Resistance	R	$E/R = I$	Ohma	"
Quantity of Electricity × turns					"
Quantity of Magnetism × turns	Linkage	T	$T = P \times \text{turns}$	Maxawelma	"
Linkage ÷ current	Inductance	L	$L = T \div I$	Henryma	"
Imaginary Resistance due to Capance	Electric Reactance	R_K	$R_K = 1 \div K\omega$	Ohmak	"
Recip. of do.	Electric Permittance	S_K	$S_K = K\omega$	Momak	"
Imaginary Resistance due to Inductance	Magnetic Reactance	R_M	$R_M = L\omega$	Ohmam	"
Recip. of do.	Magnetic Permittance	S_M	$S_M = 1 \div L\omega$	Momam	"
Total Imaginary Resistance	Reactance	R_j	$R_j = L\omega + 1/K\omega$	Ohmaj	"
Recip. of do.	Permittance	S_j	$S_j = K\omega/(KL\omega^2 + 1)$	Momaj	"
Apparent Resistance	Impedance	R_v	$R_v = \sqrt{R^2 + (L\omega - 1/K\omega)^2}$	Ohman	"
Recip. of do.	Admittance	S_v	$S_v = 1/\sqrt{R^2 + (L\omega + 1/K\omega)^2}$	Moman	"
Increase per degree C.	Temperature Coeff.	σ_E, ρ_E			

HEAT.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Unit.*
Quantity	Heat	Q	$Q=KE$	Erg.
Quantity per cc.	Heat Density	C	$C=Q\div V$	Ergav
Quantity Coefficient	Specific Heat	k	$kEV=Q$	
Quantity Factor	Heat Capacitance	K	$k\times V=Q$	
Potential Difference	Temperature Difference	E	$Ek=Q$	Degree
Potential Difference per cm.	Temperature Gradient	F	$E\div L=F$	Degreel
Quantity per sec.	Flux	I	$I=Q/T$	Ergat
Quantity per sq. cm.	Flux Density	J	$J=I\div A$	Ergart
Flux Coefficient	Impartivity	s	$Fs=J$	
Recip of do.	Morativity	r	$F/r=J$	
Flux Factor	Impartance	S	$ES=I$	Foura
Recip of do.	Moratance	R	$E/R=I$	Ruofa
Diffusion Coefficient	Diffusivity	s_E	$s_E=s\div k$	
Quantity Coefficient Constant Volume	Specific Heat, Constant Volume	k_v		
Quantity Coefficient Constant Pressure	Specific Heat, Constant Pressure	k_p		
Critical Constants	Critical Temperature	E_c		
	Critical Pressure	Π_c		
	Critical Volume	V_c		
Coefficient of	Linear Expansion	L_E		
" "	Surface "	A_E		
" "	Volume "	V_E		

RADIATION.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Unit.*
Quantity of Radiation Emitted	Radiance	$\mathfrak{Q}$	$g\mathfrak{Q} = \mathfrak{P}$	Erg.
Quantity per sq. cm.	Radivity	$\mathfrak{D}$	$\mathfrak{D} \times \text{area} = \mathfrak{Q}$	Ergar
Quantity of Light Emitted	Lumenescence	$\mathfrak{P}$	$\mathfrak{P} = g\mathfrak{Q}$	
Quantity per sq. cm.	Lumenosity	$\mathfrak{B}$	$\mathfrak{B} \times \text{area} = \mathfrak{P}$	
Radiance per Degree C.	Emittance	$\mathfrak{K}$	$\mathfrak{K}\mathfrak{E} = \mathfrak{Q}$	
Radiance per sq. cm.	Emissivity	$\mathfrak{k}$	$\mathfrak{k}\mathfrak{E} = \mathfrak{D}$	
Lumenance per Degree C.	Lumenance	$\mathfrak{M}$	$\mathfrak{M}\mathfrak{E} = \mathfrak{P}$	
Lumenance per sq. cm. per Degree C.	Lumenity	$\mathfrak{m}$	$\mathfrak{m}\mathfrak{E} = \mathfrak{B}$	
Radiance Received	Receptance	$\mathfrak{q}$	$\mathfrak{e}\mathfrak{q} = \mathfrak{J}$	
Radiance per sq. cm.	Receptivity	$\mathfrak{d}$	$\mathfrak{d} \times \text{area} = \mathfrak{q}$	
Lumenescence Received	Luxance	$\mathfrak{p}$	$\mathfrak{f}\mathfrak{p} = \mathfrak{A}$	
Lumenescence per sq. cm.	Luxity	$\mathfrak{b}$	$\mathfrak{b} \times \text{area} = \mathfrak{p}$	
Radiance Absorbed	Absorbance	$\mathfrak{J}$	$\mathfrak{J} = \mathfrak{e}\mathfrak{q}$	
Radiance per sq. cm.	Absorbity	$\mathfrak{I}$	$\mathfrak{I} \times \text{area} = \mathfrak{J}$	
Lumenescence Absorbed	Illumance	$\mathfrak{A}$	$\mathfrak{A} = \mathfrak{f}\mathfrak{p}$	
Lumenescence per sq. cm.	Illumity	$\mathfrak{V}$	$\mathfrak{V} \times \text{area} = \mathfrak{A}$	
Absorbity ÷ Receptivity	Absorbance Efficiency	$\mathfrak{e}$	$\mathfrak{e} = \mathfrak{J} \div \mathfrak{d}$	
Illumity ÷ Luxity	Luxance Efficiency	$\mathfrak{f}$	$\mathfrak{f} = \mathfrak{V} \div \mathfrak{b}$	
Lumenity ÷ Radivity	Luminous Efficiency	$\mathfrak{g}$	$\mathfrak{g} = \mathfrak{B} \div \mathfrak{P}$	

GRAVITY.

Nature.	*Symbol.*	*Nature.*	*Symbol.*
Gravity	G	Acceleration of Gravity	i
Quantity Coefficient	γ	Ponderate Acceleration (note)	g

MECHANICS.

Nature.	*Symbol*	*Nature.*	*Symbol.*
Mass	M	Angular Velocity	ω
Length	L	Work	Ψ
Area	R	Work per cm. or Force	Φ
Volume	V	Work per sq. cm. Surface Tension	Σ
Density	ς	Work per c. c. or Pressure	Ξ
Velocity	v	Work per second	Π
Periodicity	τ	Force per cm.	Γ
Acceleration	a	Young's Modulus	ζ
Plane Angle	φ, θ	Rigidity	ξ
Solid Angle	Ω	Tensile strength	χ

NOTE.—*I. e.*, acceleration due to gravity plus that due to rotation of earth on its axis, around the sun, etc.

THERMODYNAMIC NOTES (NO. 10): REVISION OF SOME THERMAL DATA CONCERNING BENZENE.[1]

K. TSURUTA.

PROFESSOR S. YOUNG[2] has given full measurements on vapor pressure (p) and specific volumes (v_l liquid and v_g vapor) of benzene in its saturated states. The values of $\frac{dp}{dT}$ were found from Biot's formula used by him, and the heat of vaporization (r or r_{lg}) was calculated, the results being given in the following table, in which for reference, some of the original data are reproduced.[3]

HEAT OF VAPORIZATION r (ACCORDING TO PROFESSOR S. YOUNG).

t	v_l	v_g/v_l	p	r	t	v_l	v_g/v_l	p	r
	c.c.		m.m.	Cal.		c.c.		m.m.	Cal.
80°C.	1.2278	298.0	753.62	95.34	180°C.	1.4480	27.90	7625.2	74.72
90	1.2436	225.0	1016.1	94.45	190	1.4798	22.70	9049.4	71.89
100	1.2615	169.0	1344.3	91.60	200	1.5139	18.65	10663.0	69.01
110	1.2806	129.0	1748.2	89.02	210	1.5546	15.23	12482.0	65.75
120	1.3000	100.0	2238.1	86.41	220	1.5986	12.47	14526.0	62.23
130	1.3214	79.4	2824.9	84.78	230	1.6487	10.15	16815.0	58.44
140	1.3440	62.7 63.60	3520.0	82.06 83.25	240	1.7091	8.21	19369.0	54.13
150	1.3680	50.5 50.90	4334.8	80.17 80.82	250	1.7827	6.55	22214.0	49.36
160	1.3918	42.10	5281.9	80.31	260	1.8770	5.14	25376.0	43.79
170	1.4198	34.30	6374.1	78.13	270	2.0063	3.88	28885.0	36.90

We reproduce again the direct measurements of r by Griffiths and Marshall :[4]

---

[1] It may be remarked that this revision was prepared some years ago for some cryoscopic experimental investigations with benzene which I had intended to carry on.

[2] S. Young, *Jour. of Chem. Society*, Vol. LV., (1899) and Vol. LIX. (1891).

[3] The unit quantity of heat here adopted is about 4.189×10^6 ergs (A. Griffiths, *Phil. Mag.*, Vol. XL. (1858), p. 453).

[4] A. Griffiths and Marshall, *Phil. Mag.*, Vol. XLI. (1896).

HEAT OF VAPORIZATION r (ACCORDING TO GRIFFITHS AND MARSHALL).

t (Nitrogen Scale).	r Cal.	t (Nitrogen Scale).	r Cal.
19°.947C.	103.82	40°.045C.	100.71 ±0.057
30 .066	102.30 ±0.076	50 .014	99.14 ±0.034

The authors give the following linear formula as showing a very close approximation to the experimental values:

$$r = 107.05 - 0.1581 \times t.$$

When we plot these values (indicated by "experi." in the following table), it will be found that between 180° C. and 270° C. the representative points are regularly arranged, but quite irregularly between 80° C. and 180° C., showing, perhaps, that with Professor Young's sealed tube method "the results are more reliable at high than at low temperature." (Professor Young). Connecting them, however, by a mean curve I obtained the numbers (indicating by "curve") in the following table:

HEAT OF VAPORIZATION r (ACCORDING TO WRITER'S MEAN CURVE).

t	r (Curve.)	r (Experi.)	t	r (Curve.)	r (Experi.)	t	r (Curve.)	r (Experi.)
	Cal.	Cal.		Cal.	Cal.		Cal.	Cal.
10°	105.24	———	100°	91.05	91.60	190°	71.93	71.89
20	103.75	103.82	110	89.20	89.02	200	69.01	69.01
30	102.26	102.30	120	87.36	86.41	210	65.80	65.75
40	100.76	100.71	130	85.43	84.78	220	62.32	62.23
50	99.24	99.14	140	83.48	82.06 83.25	230	58.49	58.44
60	97.65	———	150	81.35	80.17 80.82	240	54.21	54.13
70	96.04	———	160	79.20	80.31	250	49.40	49.36
80	94.40	95.34	170	76.90	78.13	260	43.76	43.79
90	92.76	94.45	180	74.53	74.72	270	36.97	36.90

It will be seen from this table that my mean curve can be used for the purpose of interpolation and hence for some other purposes.

From these values of r we can calculate backwards the ratio of $\frac{v_t}{v_s}$ and then v_s taking as correct Professor Young's experimental values of v_i. I did this and found that the "mean values" of densities of the liquid and vapor obey in quite a satisfactory manner the empirical "law of straight diameter" (Cailletet and Mathias), not only from 80° C. to the critical temperature as remarked by Professor Young himself, but also from 80° C. down to the triple point. A more minute examination showed, however, the so-called diameter, in the case of *my* mean curve, to consist of two nearly straight parts meeting somewhere near the temperature 180° C., or more strictly, to be a curve whose curvature is so slight that it can be so described. This may indicate that *my* mean curve is not a good mean curve. But since we should ascribe much importance to the experimental values of Griffiths and Marshall, and surely to those that have been deduced from Professor Young's measurements between 180° C. and 270° C., I think that we must rather regard the empirical "law of straight diameter" as holding good in first approximation, but not in so reliable a degree as to draw from it any thermodynamic conclusions concerning benzene.

Another important source of information is the formula given by Regnault for the total heat (λ):

$$\lambda = 109.0 + 0.24429 \times t - 0.0001315 \times t^2.$$

To make it comparable with the above results of calculation, and then to make some applications of it, it must first of all be subject to some thermometric and calorimetric reductions. Then by interpolations it would become possible to calculate, if we make use of the above result, the specific heat of saturated vapor, and to obtain perhaps quite a satisfactory formula for the specific heat of liquid benzene at higher temperatures; or to calculate, if we take the formula in conjunction with some reliable measurements on the specific heat of liquid benzene, the heat of vaporization at various temperatures, and then see how far do the results coincide with the calculations of this kind given by Wüllner.[1] As, however, I do not possess Regnault's original memoirs, I was unable to examine these points.

---

[1] A. Wüllner, Experimental Physik, Bd. III. (4to Auf.), 5, 726.

Let us now turn to the vapor pressures of benzene below its triple point (+ 5°.3 C.).

The measurements of these by Fischer,[1] Ramsay and Young,[2] and Ferche[3] were plotted on section paper and mean curves drawn to pass well through the representative points. This was done twice quite independently, and when the results obtained were compared, it was found that for liquid benzene they differed very little in the cases of Fischer's and Ferche's measurements, generally by 0.01–0.03 mm. of mercury; but with Professor Ramsay and Young's they differed sometimes by 1.5 mm., perhaps owing to a comparatively small number of points available for drawing the mean curve between the triple point and — 1° C.

VAPOR PRESSURE OF LIQUID BENZENE.

t	Fischer.	R. & Y.	Ferche.	Mean Curve.	*t*	Fischer.	R. & Y.	Ferche.	Mean Curve.
	m.m.	*m.m.*	*m.m.*	*m.m.*		*m.m.*	*m.m.*	*m.m.*	*m.m.*
— °1	24.87	25.04	25.03	24.98	+2°.5	30.28	30.25	30.37	30.30
—0 .6	25.60	25.73	25.68	25.67	3	31.11	31.11	31.23	31.55
0	26.34	26.42	26.41	26.39	3	31.99	32.00	32.10	32.03
+0 .5	27.10	27.14	27.16	27.13	3 .5	32.87	32.94	33.01	32.94
1	27.88	27.88	27.91	27.89	4	33.78	33.89	33.94	33.87
1 .5	28.66	28.65	28.69	28.67	4 .5	34.70	34.88	34.87	34.82
2	29.46	29.43	29.52	29.47	5				

Proceeding in a similar way with solid benzene, I obtained the following values from the different authors' sets of measurements. Things are less favorable in the present case than in the preceding, in fact, not only did my own mean curves, several of which I drew for one set, differ much among themselves in the cases of Professor Ramsey and Young's and Fischer's measurements, but also the different sets compared with one another showed greater discrepancies than in the preceding case. It will be seen that my mean curve runs on the whole quite close to Ferche's, that of Fischer lying nearly as much above the latter as Professor Ramsey and Young's lies below it.

[1] W. Fischer, *Wiede. Ann.*, Bd. 28 (1886).

[2] W. Ramsay and S. Young, *Phil. Mag.*, Vol. 23 (1887).

[3] J. Ferche, *Wiede. Ann.*, Bd. 44 (1891).

VAPOR PRESSURE OF SOLID BENZINE.

t	Fischer.	R. & Y.	Ferche.	Mean Curve.	*t*	Fischer.	R. & Y.	Ferche.	Mean Curve.
	m.m.	*m.m.*	*m.m.*	*m.m.*		*m.m.*	*m.m.*	*m.m.*	*m.m.*
−1°C.	23.21	21.97	22.52	22.57	+2°.5C	29.30	28.97	29.14	29.14
−0.5	24.03	22.94	23.40	23.46	3	30.25	30.05	30.19	30.16
0	24.88	23.90	24.30	24.36	3.5	31.25	31.16	31.27	31.23
+0.5	25.72	24.89	25.23	25.28	4	32.28	32.32	32.36	32.32
1	26.58	25.88	26.18	26.21	4.5	33.35	33.52	33.50	33.46
1.5	27.46	26.89	27.13	27.16	5	34.50	34.75	34.68	34.64
2	28.36	27.91	28.12	28.13					

Following the line of reasoning used by Guldberg[1] we can obtain a thermodynamic relation between p_i and p_s. By the fundamental principles of reversible thermodynamics considered as applicable to a substance in any of the two coexisting phases of it, we have the well known formulæ: in the case of liquid and vapor phases.

$$r_{gi} = T(v_g - v_i)\cdot\frac{dp_i}{dT},$$

and

$$h_{gi} = T.\,d\frac{\left(\frac{r_{gi}}{T}\right)}{dT} + c_i;$$

in the case of vapor and solid phases,

$$r_{gs} = T(v_g - v_s)\cdot\frac{dp_s}{dT},$$

and

$$h_{gs} = T.\,d\frac{\left(\frac{r_{gs}}{T}\right)}{dT} + c_s,$$

where h_{gl} (h_{gs}) and c_l (c_s) develop respectively the specific heats along vapor- and liquid- (solid-) lines. Assuming the vapor over the liquid and solid states to be the same, we may put

$$h_{gi} = h_{gs},$$

so that

$$c_i - c_s + T\frac{d}{dT}\left(\frac{r_{gi} - r_{gs}}{T}\right) = 0.$$

By integration we obtain

$$\int\frac{c_i - c_s}{T}\,dT = \frac{r_{gi} - r_{gs}}{T} + k \text{ (const.)}.$$

---

[1] Guldberg, C. R. (1870).

Assuming $c_l - c_s$ as constant we obtain

$$(c_i - c_s)\log T = \frac{r_{gs} - r_{gi}}{T} + k,$$

which gives $$(c_i - c_s)\log\frac{T_0}{T} = \frac{F_0}{T_0} - \frac{1}{T} r_{gs} - r_{gi}),$$

where $F_0(= r_{gs} - r_{gi}$ exactly at the triple point) denotes the heat of fusion at that temperature. When we neglect v_i as well as v_s in comparison with v_g and make the further assumption that Boyle-Gay-Lussac's law together with Avogadro's "hypothesis" holds good for the vapor of benzene between the triple point and near 0°C., we obtain

$$\frac{1.971}{78}.\ T.\ \frac{d}{dT}\left(\log\frac{p_s}{p_i}\right) = \frac{F_0}{T_0} - (c_i - c_s)\log\frac{T_0}{T}\ .$$

Hence, by integration and some transformation,

$$\log\frac{p_s}{p_i} = \frac{78}{1{,}971}.\ \log\frac{T.}{T_0}\ .\ \left(\frac{F_0}{T_0} + \frac{c_i - c_s}{2}\log\frac{T}{T_n}\right),$$

which becomes, when we make some neglections,

$$\log\frac{p_s}{p_i} = \frac{78}{1{,}971}\ .\ \frac{\tau\ .\ F_0}{T_0^2}\ ,$$

exactly as given by Guldberg.

In the last but one formula I have put

$$\begin{aligned} T_0 &= 278.3, \\ F_0 &= 30.00, \\ \left.\begin{aligned} c_i &= 0.3350, \\ c_s &= 0.2032 \end{aligned}\right\} & \text{ (Ferche.)}^1 \end{aligned}$$

and obtained the numbers given in the first three columns of the following table:

---

[1] Here we may put c_l and c_s equal to the specific heats at constant pressure of liquid and solid benzene. M. Fischer puts the specific heat of solid benzene equal to 0.319, while M. Ponsot (C. R., 1894) adopts the value 0.017 of the difference of the two specific heats without giving any reason for it. Though of no serious effect in the present case, yet such a difference considered in itself requires an experimental examination.

Vapor Pressures of Solid and Liquid Benzene.

t	p_s	$\frac{p_l}{p_s}$	p_s (Fischer.) minus	p_s (R. & Y.) minus	p_s (Ferche) minus	p_s (Mean.) minus
	Calculated.	Calculated.	p_s (Calc.)	p_s (Calc.)	p_s (Calc).	p_s (Calc.)
	m.m.		*m.m.*	*m.m.*	*m.m.*	*m.m.*
−1° C.	22.69	1.1011	+0.52	−0.72	−0.17	−0.12
−0 .5	23.49	1.0927	+0.54	−0.55	−0.09	−0.03
0	24.34	1.0844	+0.53	−0.44	−0.04	+0.02
+0 .5	25.21	1.0761	+0.51	−0.32	+0.02	+0.07
1	26.11	1.0680	+0.47	−0.23	+0.07	+0.10
1 .5	27.05	1.0599	+0.41	−0.16	+0.08	+0.11
2	28.01	1.0518	+0.35	−0.10	+0.11	+0.12
2 .5	29.03	1.0438	+0.27	−0.06	+0.11	+0.11
3	30.07	1.0359	+0.18	−0.02	+0.12	+0.09
3 .5	31.16	1.0280	+0.09	0	+0.11	+0.07
4	32.29	1.0201	−0.01	+0.03	+0.07	+0.03
4 .5	33.46	1.0123	−0.01	+0.06	+0.04	0
5	34.66	1.0046	−0.16	+0.09	+0.02	−0.02

The numbers in the last four colums may be taken as indicating teat the several assumptions we have made in deducing the formula employed as well as the several experimental data we have used in calculating the vapor pressures, are both of them not very wrong. Hence the formula with the data may be used as one of first approximation.[1]

Tokyo, Japan.

---

[1] By the way, let me here add the following. I obtained the following values of the constants of Biot's form of interpolation formula; choosing the mean values of pressure at $-0^\circ.5$, $+2^\circ$, and $+4^\circ.5$ C. for liquid benzene:

$$\log p_l = a + b\alpha^t,$$

$$a = \overline{7}.8749340,$$

$$\log b = 0.8770540,$$

$$\log \alpha = 0.0013878.$$

Similarly for solid benzenes:

$$\log p_s = a' + b'\rho^t$$

$$a' = 3.153721,$$

$$\log b' = 0.2512469,$$

$$\log \beta = \overline{1}.9921452.$$

AN EFFICIENT BOYLE'S-LAW APPARATUS.

W. J. Humphreys.

NOTHING is more discouraging to teacher or student than a piece of apparatus incapable of giving consistent and approximately accurate results. For this reason the ordinary U shaped tube, used for demonstrating the truth of Boyle's law, is one of the least satisfactory things the junior laboratory student has to work with. No matter how careful his measurements, irregularities in the bore of the tube, even where all other sources of appreciable error are avoided, will lead to irregular and unsatisfactory determinations.

The importance of apparatus capable of yielding good results, however elementary the experiments, is my excuse for offering the following description of a Boyle's law device which I have recently had constructed, and which, according to tests, readily gives values that do not differ from the correct ones by more than one part in a thousand.

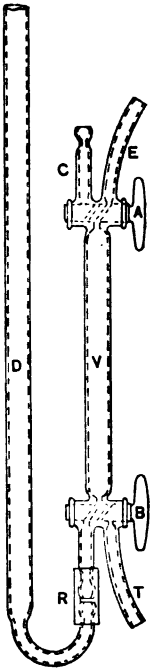


Fig. 1.

Referring to the figure : *V* is a tube about twelve centimeters long and one centimeter in diameter having a two-way stop-cock at eachend. *D* is a long tube of about the same diameter as *V* to avoid any appreciable difference in capillary action. *D* may be sealed to *V* or connected to it in any way desired—the figure shows the method I have adopted. In the event of any leakage the short rubber tube *R* may be made tight either by wrapping or by putting it on the glass tubes with shellac varnish. However I have not found either of these precautions necessary.

The stop-cocks, of course, must be air tight. An excellent lubricant for them is paraffine, with just enough vaseline added to keep it from being brittle.

The operation is as follows: Close stop-cock B and fill D to any desired height with mercury, then open B and allow a small amount of mercury to run into V, then turn B till V and T are in communication. This lets the mercury out of V but leaves the tube through B that connects V and D entirely filled. Now close A and the volume of air in V is perfectly definite and at atmospheric pressure. Next turn B till V and D are again in communication. The mercury will now rise slowly in V and be free from all air bubbles. When the mercury has come to rest close B and read the difference in mercury levels in D and V, either with a cathetometer or with a meter bar placed between D and V. Now open A and B and run the mercury out T and weigh it. Obtain similar results for as many different heights as desired.

Lastly leave V open to E say and run mercury up V and into A, close B, open V to C, then run the mercury out T and weigh. This gives the weight of the mercury necessary to fill V absolutely.

Let $W =$ weight of mercury required to fill V,
" $w =$ " " " run in when A is closed,
" $p =$ difference in heights of mercury in D and V,
" $B =$ height of barometer at time of experiment,
" $V =$ volume of tube V,
" $v =$ " " compressed gas in V.

Then

$$\frac{W}{W-w} = \frac{V}{v} = \frac{B+p}{B},$$

and

$$B = \frac{p(W-w)}{w}.$$

Of course B may be obtained directly from a barometer, or it can be determined to within a fraction of a millimeter by the method just shown, its value substituted in the equations, and Boyle's law tested.

This is one method, and occasion to use it might arise, of obtaining a fairly satisfactory barometric reading without the use of a standard barometer. I have described the apparatus as used for pressures above one atmosphere. It can be used, however, with equal ease and accuracy for pressures less than atmospheric, but the operations are so obvious and simple that a description of them would be superfluous, except possibly the statement that it is necessary to determine the weight of the mercury required to fill the tube in B that connects V with T.

If the student has time he may simply assume that, at constant temperature, the volume of a gas is some fixed function of the pressure. His values will show him what this function is, determine the height of the barometer, and in every way give him a most satisfactory demonstration of the truth of Boyle's law.

It may be not out of place to say that in filling D it is advisable to introduce the mercury through a small funnel or thistle tube, with its stem drawn out to a fine opening. This prevents fouling the walls of the tube, the dirt being collected on the sides of the funnel.

ROUSS PHYSICAL LABORATORY, UNIVERSITY OF VIRGINIA, Dec. 7, 1899.

NEW BOOKS.

Strahlung und Temperatur der Sonne. By Dr. J. Scheiner. Pp. i–v, 1–99. W. Engelmann, Leipzig, 1899.

This monograph on solar temperatures shows the importance which is now attached to Stefan's law, particularly in Germany. Some time has elapsed since Stefan noticed that the observations of Dulong and Petit, de la Provostage and Desains, Draper, Ericsson and others could be satisfactorily interpreted by assuming the quantity of heat radiated by a body to be proportional to the fourth power of its absolute temperature. Boltzmann thereafter deduced the same law from the electromagnetic theory of light by aid of the second law of thermodynamics. Boltzmann himself insisted on the provisional character of his proof; but the law has recently received independent experimental confirmation at the hands of Lummer and Pringsheim (1560° C.) and others, operating with absolutely black bodies, *i. e.*, with hollow equally heated radiators emitting heat from within. For such bodies only the law appears to be valid.

Dr. Scheiner, therefore, assumes the truth of Stefan's law to this extent, and defines the sun's effective temperature, as the temperature which it would have if it radiated like a black body. From the known value of the solar constant, s (heat received per sq. cm., per sec.) and the known heat emission, h, of a black body for a definite temperature interval (100°, Kurlbaum and others), it is thus possible to arrive at trustworthy values for the temperature of the sun t. If φ be the apparent solar diameter, Stefan's law leads approximately to

$$t = 273 \sqrt[4]{\frac{2.843}{\sin^2\frac{\varphi}{2}} \frac{s}{h}},$$

and Scheiner in this way finds $t = 7010°$. Treating all earlier observations by the same method he computes the following list of independent values:

Pouillet,	$t = 5600°$	Langley,	$t = 6000°$
Secchi,	5400°	Wilson & Gray,	6200°
Violle,	6200°	etc.	
Soret,	5500°		

In view of the enormous discrepancies in the values deduced for the sun's temperature by these observers themselves (data between 1500° and

10,000,000°), the author concludes that the consistent values of the present method must commend the problem to the serious consideration of astronomers. The reviewer fails to see however, why similar data similarly interpreted, do not necessarily lead to comparable results.

Corrected for solar absorption, Scheiner's values for the effective temperature of the sun's photosphere is 7760° C.

Among many indirect methods for estimating the sun's temperature, the one based on the position (λ) of the energy maximum in the solar spectrum deserves mention here. Langley's observations (maximum at $\lambda = 0{\cdot}6\mu$) interpreted by Wien put the sun's effective temperature at 4630°, in reasonable agreement with the above data.

The monograph contains a full digest of correlative data in solar physics. The absorption of the earth's atmosphere is treated in detail. A complete account is given of solar radiation, considered both photometrically and thermally, including a summary of the laws of radiation hitherto proposed. Much space is devoted to the distribution of light over the sun's disc, to the absorption of the solar atmosphere, to secular and periodic changes of the sun's temperature, to the electrodynamic radiation of the sun, etc. Any student interested in the profound cosmical problems treated will profit by the varied information given in this useful little book.

CARL BARUS.

Geschichte der Physikalischen Experimentierkunst. By E. GERLAND UND F. TRAUMÜLLER. Pp. 442. (14 M.; bound, 17 M.). W. Engelmann, Leipzig, 1899.

The general history of physics embraces not only physical theories, but also the discovery of physical facts upon which the theories rest and the art of experimentation, by which the facts were ascertained. The above book limits itself to the last of these functions. The authors trace the history of the art of experimentation from early times down to the beginning of the present century. The 425 figures show some ingenious devices which are not usually explained in historical works. Here we find illustrated the ancient Egyptian process of glass-blowing, the Archimedian endless screw, several of Hero's curious devices, the famous "tower of the winds" in Athens, which testifies to an early interest in meteorology, the Roman steelyards, the hydrometer of Synesius, Al Khâzinî's "balance of wisdom," Gilbert's "instrumentum declinationis," Galileo's pendulum clock, Guericke's quaint illustrations of experiments on atmospheric pressure, Agricola's and Papin's mine ventilators, and many kinds of apparatus devised during more recent times. The work of the nineteenth century remains untouched, except the researches on electricity and magnetism during the first forty years.

The experiments of the *Accademia del Cimento* of Florence are described with greater fullness than we have seen in other histories. Extremely interesting are the series of experiments on the compressibility of water, culminating in the well-known test with the hollow spheres of silver which were filled with water, closed tight, and then disfigured by hammering until the water forced its way through the pores (or cracks?) of the metal. That the Academicians did not feel justified by their experiments to draw the conclusion that water is incompressible, speaks highly for the caution which they exercised in their researches. Their report on the subject ends as follows: "It is certain that, as compared with air, water withstands an infinitely greater pressure (if we may so express ourselves), which confirms what we said at the beginning. While experimental research does not always lead us to the most remote truths which we seek, it gives us, nevertheless, in each favorable or unfavorable case, some light, be it ever so little."

The struggle for supremacy between alcohol and mercury as thermometric substances is told in an attractive manner, but the authors fail to record the interesting fact brought to light by M. Maze (*Comptes Rendus*, Vol. 120, 1895, p. 732), that in 1559 more than half a century before Fahrenheit, a mercury thermometer was constructed by the astronomer Boullian.

The learned German authors point to Seebeck as one who approached closely to the discovery of Ohm's Law three years before Ohm made his experiments in 1826, but they have overlooked the fact established by Maxwell (*Electrical Researches of the Hon. Henry Cavendish*, 1879, p. LIX.), that Cavendish as early as 1781 completed a series of experiments which amount to an anticipation of Ohm's Law.

The authors describe the electrical researches of Faraday, but do not mention Joseph Henry, who, while a teacher at the Albany Academy, made the discovery of magneto-electricity about the same time as Faraday, and who antedated Faraday in the discovery of self-induction. The authors also speak of Morse without mentioning Henry, although it is quite firmly established that Morse's assistant, Dr. Gale, applied the principle discovered by Henry to render Morse's machine effective at a distance. (*Smithsonian Report* 1857, pp. 99–106.)

But these are minor blemishes. The work as a whole is accurate and up-to-date in the information which it imparts. It contains much material otherwise difficult of access.

FLORIAN CAJORI.

COLORADO COLLEGE, COLORADO SPRINGS.

Standard Text-Books on General Physics

Elements of Theoretical Physics

By **DR. C. CHRISTIANSEN**
Professor of Physics, University of Copenhagen
TRANSLATED INTO ENGLISH BY
W. F. MAGIE, Ph.D., Professor of Physics, Princeton University
Cloth. 8vo. $3.25, net

Problems and Questions in Physics

CHARLES P. MATTHEWS, M.E., AND **JOHN SHEARER, B.S.**
Purdue University — *Cornell University*
8vo. Cloth. pp. 247 + 4. Price $1.60, net

An Intermediate Course of Practical Physics

By **A. SCHUSTER, Ph.D., F.R.S.,** AND **C. H. LEES, D.Sc.**
Owens College, Manchester — *Owens College, Manchester*
12mo. Cloth. pp. xv + 248. Price $1.10, net

Lessons on Elementary Practical Physics

By **BALFOUR STEWART, A.M., LL.D., F.R.S.,** AND **W. W. HALDANE GEE**

Vol. I. **General Physical Processes.** 12mo. $1.50, *net*
Vol. II. **Electricity and Magnetism.** $2.25, *net*
Vol. III. Part I. **Practical Acoustics.** $1.10, *net*
Part II. **Heat and Light.** *In Press*

Laboratory Manual of Physics and Applied Electricity

Arranged and Edited by EDWARD L. NICHOLS
Professor of Physics in Cornell University
IN TWO VOLUMES
Vol. I. Cloth. Price $3.00, net

JUNIOR COURSE IN GENERAL PHYSICS. By ERNEST MERRITT and FREDERICK J. ROGERS.

Vol. II. Cloth. pp. 444. Price $3.25, net

SENIOR COURSES AND OUTLINES OF ADVANCED WORK By GEORGE S. MOLER, FREDERICK BEDELL, HOMER S. HOTCHKISS, CHARLES P. MATTHEWS, and THE EDITOR.

A large proportion of the students for whom primarily this Manual is intended are preparing to become engineers, and special attention has been devoted to the needs of that class of readers. In Vol. II., specially, a considerable amount of work in applied electricity, in photometry, and in heat has been introduced.

THE MACMILLAN COMPANY
New York: 66 Fifth Avenue
BOSTON CHICAGO SAN FRANCISCO

STANDARD BOOKS ON ELECTRICITY AND MAGNETISM

The Theory of Electricity and Magnetism

By A. G. **WEBSTER**, A.B. (Harv.), Ph.D. (Berol.)

Assistant Professor of Physics, Director of the Physical Laboratory, Clark University, Worcester, Massachusetts

8vo. Cloth. pp. xii + 576. Price, $3.50, net

Elementary Lessons in Electricity and Magnetism

By **SILVANUS P. THOMPSON**, D.Sc., B.A., F.R.A.S.

New Edition. With Illustrations. 16mo. Cloth. $1.40, net.

"I consider Thompson the best book of its size and nature on the subject of electricity, and have had frequent occasion to recommend it to friends and students. It is the only book that is not simply a earrangement or reproduction of the old treatise."

—William A. Hallock, Ph.D., *Columbia University, New York City.*

The Principles of the Transformer

By **FREDERICK BEDELL**, Ph.D.

Assistant Professor of Physics in Cornell University

8vo. Cloth. 250 Illustrations. Price, $3.25, net

This work constitutes a systematic treatise on the alternating Current Transformer, and a logical exposition of the principles involved, of the most modern methods of transformer design, construction, and testing, and of transformer systems of distribution.

A Text-Book on Electro-Magnetism

AND THE CONSTRUCTION OF DYNAMOS

By **DUGALD C. JACKSON**, B.S., C.E.

Professor of Electrical Engineering, University of Wisconsin

12mo. Cloth. Price, $2.25, net

Alternating Currents and Alternating Current Machinery

By **DUGALD C. JACKSON**, C.E. and **JOHN P. JACKSON**, M.E.

University of Wisconsin — *Pennsylvania State College*

12mo. Cloth. pp. xvii + 729. Price, $3.50, net

"I like the way in which they develop their theory and make practical applications. I feel sure at the book will receive a hearty welcome."

—Geo. W. Patterson, Jr., *Asst. Professor of Physics, University of Michigan*

The Alternating Current Circuit

By **W. P. MAYCOCK**, M.T.E.E.

An Introductory and Non-Mathematical Book for Engineers and Students. With 51 Illustrations, Index, and Ruled Pages for Notes.

16mo. Cloth. pp. viii + 102. Price, $1.00

The Storage Battery

A Practical Treatise on the Construction, Theory and Use of Secondary Batteries,

By **AUGUSTUS TREADWELL**, Jr.

Cloth. 12mo. pp. xx + 257. Price, $1.75, net

JUST READY

Electricity in Town and Country Houses

By **PERCY E. SCRUTTON.**

Second Edition. Fully Illustrated. 12mo. Cloth. pp. xii + 148. Price, $1.00.

A brief and simple statement of the first principles of the application of electricity to the needs of the home, for heating, cooking, lightning, etc. A description is given of the plants required for towns of various size, and there are numerous illustrations showing examples of electric appliances. It is a popular and untechnical book, and intended for those who have not the time necessary for a study of more strictly scientific works.

THE MACMILLAN COMPANY

New York: 66 Fifth Avenue

BOSTON CHICAGO SAN FRANCISCO

THE CHEAPEST BOOKS IN

Science, Philosophy and Psychology

Now Publishing in America

High Grade Paper. **Large Print.** **Stitched Backs**

THE RELIGION OF SCIENCE LIBRARY

No. 1. **The Religion of Science.** By PAUL CARUS. 25 cents (1s. 6d.).
2. **Three Introductory Lectures on the Science of Thought.** By F. MAX MÜLLER. 25 cents (1s. 6d.).
3. **Three Lectures on the Science of Language.** By F MAX MÜLLER. 25 cents (1s. 6d.).
4. **The Diseases of Personality.** By TH. RIBOT. 25 cents (1s. 6d.).
5. **The Psychology of Attention.** By TH. RIBOT. 25 cents (1s. 6d.).
6. **The Psychic Life of Micro-Organisms.** By A. BINET. 25 cts. (1s. 6d.).
7. **The Nature of the State.** By PAUL CARUS. 15 cents (9d.).
8. **On Double Consciousness.** By ALFRED BINET. 15 cents (9d.).
9. **Fundamental Problems.** By PAUL CARUS. Pp., 373. 50 cents (2s. 6d.).
10. **The Diseases of the Will.** By TH. RIBOT. 25 cents (1s. 6d.).
11. **The Origin of Language** and **The Logos Theory.** By LUDWIG NOIRÉ. 15 cents (9d.).
12. **The Free Trade Struggle in England.** By GEN. M. M. TRUMBULL. 25 cents (1s. 6d.).
13. **Wheelbarrow on the Labor Question.** 35 cents (2s.).
14. **The Gospel of Buddha.** By PAUL CARUS. 35 cents (2s.).
15. **Primer of Philosophy.** By PAUL CARUS. 25 cents (1s. 6d.).
16. **On Memory,** and **The Specific Energies of the Nervous System.** By PROF. EWALD HERING. 15 cents (9d.).
17. **The Redemption of the Brahman.** By R. GARBE. 25 cents (1s. 6d.).
18. **An Examination of Weismannism.** G. J. ROMANES. 35 cents (2s.).
19. **On Germinal Selection.** By AUGUST WEISMANN. 25 cents (1s. 6d.).
20. (Out of print.)
21. **Popular Scientific Lectures.** By ERNST MACH. 50 cents (2s. 6d.).
22. **Ancient India. Its Language and Religions.** By PROF. H. OLDENBERG. 25 cents (1s. 6d.).
23. **The Prophets of Israel.** By PROF. C. H. CORNILL. 25 cents (1s. 6d.).
24. **Homilies of Science.** By DR. PAUL CARUS. 35 cents (2s.).
25. **Thoughts on Religion.** By G. J. ROMANES. 50 cents.
26. **The Philosophy of Ancient India.** By R. GARBE. 25 cents (1s. 6d.).
27. **Martin Luther.** By GUSTAV FREYTAG. 25 cents (1s. 6d.).
28. **English Secularism.** GEORGE JACOB HOLYOAKE. 25 cents.
29. **On Orthogenesis.** By TH. EIMER. 25 cents (1s. 6d.).
30. **Chinese Philosophy.** By PAUL CARUS. 25 cents (1s. 6d.).
31. **The Lost Manuscript.** By GUSTAV FREYTAG. 60 cents (3s.).
32. **A Mechanico-Physiological Theory of Organic Evolution.** CARL VON NÄGELI. 15 cents (9d.).
33. **Chinese Fiction.** By DR. GEORGE T. CANDLIN. 15 cents (9d.).
34. **Mathematical Essays and Recreations.** By H. SCHUBERT. 25 cents (1s. 6d.).
35. **The Ethical Problem.** By PAUL CARUS. 50 cents (2s. 6d.).
36. **Buddhism and its Christian Critics.** PAUL CARUS. 50 cents (2s. 6d.).
37. **Psychology for Beginners.** HIRAM M. STANLEY. 20 cents (1s.).
38. **Discourse on Method.** RENE DESCARTES. 25 cents (1s. 6d.).
39. **The Dawn of a New Era.** PAUL CARUS. 15 cents (9d.).
40. **Kant and Spencer.** PAUL CARUS. 15 cents (9d.).

THE OPEN COURT PUBLISHING COMPANY

324 Dearborn Street, Chicago, Ill.

LONDON: Kegan Paul, Trench, Trübner & Co., Ltd.

The Astrophysical Journal

AN INTERNATIONAL REVIEW OF SPECTROSCOPY AND ASTRONOMICAL PHYSICS

EDITED BY

GEORGE E. HALE
Director of the Yerkes Observatory

AND

JAMES E. KEELER
Director of the Lick Observatory

WITH THE COÖPERATION OF
THE LEADING ASTROPHYSICISTS OF THE UNITED STATES AND EUROPE

Contents of Recent Numbers

FEBRUARY 1898

On the Arc-Spectra of the Elements of the Platinum Group. I. . . . . . H. KAYSER

The Aberration of Parabolic Mirrors
CHARLES LANE POOR

On the Causes of the Sun-Spot Period
E. J. WILCZYNSKI

Radiation in a Magnetic Field
A. A. MICHELSON

Minor Contributions and Notes

Reviews

MARCH 1898

The Concave Grating for Stellar Photography
CHARLES LANE POOR and S. ALFRED MITCHELL

On Certain New Results relating to the Phenomena Discovered by Dr. Zeeman
A. CORNU

Resumé of Solar Observations made at the Royal Observatory of the Roman College during the Second Half of 1897
P. TACCHINI

On the Arc-Spectra of the Elements of the Platinum Group. II. . . . . H. KAYSER

Minor Contributions and Notes

Reviews

Recent Publications

APRIL 1898

On the Spectrum of Hydrogen in the Nebulae . . . . J. SCHEINER

On the Level of Sun-Spots and the Cause of their Darkness . . . A. L. CORTIE, S. J.

Sources of Error in Investigations on the Motion of Stars in the Line of Sight
H. C. VOGEL

The Variation of Solar Radiation
FRANK W. VERY

Arc-Spectrum of Vanadium
HENRY A. ROWLAND and CALEB N. HARRISON

Minor Contributions and Notes

Reviews

MAY 1898

Theoretical Considerations respecting the Dependence of Wave-Lengths on Pressure which Messrs. Humphreys and Mohler have observed in the Arc-Spectra of Certain Elements . . . . J. WILSING

Correspondence of the Photographic Durchmusterung with the Visual . R. H. TUCKER

The Caustic of the Right Parabolic Cylinder
C. W. CROCKETT

The Parabolic Mirror . . . . C. W. CROCKETT

Comparison of Oxygen with the extra Lines in the Spectra of the Helium Stars, B Crucis, etc.; also Summary of the Spectra of Southern Stars to the 3½ Magnitude and their Distribution . . . . FRANK McCLEAN

Arc-Spectrum of Zirconium and Lanthanum
HENRY A. ROWLAND and CALEB N. HARRISON

Minor Contributions and Notes

CHICAGO
The University of Chicago Press
LONDON: WM. WESLEY & SON

Published every month except July and September

Subscription Price
Domestic, $4.00; Foreign, $4.50

PORTRAITS

To satisfy a demand which has arisen, the publishers have arranged to issue a limited number of copies of the portraits of scientific men, that have appeared from time to time as frontispieces in the Physical Review.

The portraits are printed in photogravure, on plate paper, suitable for framing, and are for sale at the very low price of 25 cents each.

NOW READY.

JOHN TYNDALL.
HEINRICH HERTZ.
HERMAN VON HELMHOLTZ (1876).
AUGUST KUNDT.
ALFRED M. MAYER.
WILLIAM A. ROGERS.

THE PHYSICAL REVIEW

A Journal of Experimental and Theoretical Physics

CONDUCTED BY

EDWARD L. NICHOLS, ERNEST MERRITT and FREDERICK BEDELL

XLIX. November-December, 1899

TABLE OF CONTENTS

Frontispiece

On the Freezing Points of Aqueous Solutions of Non-electrolytes. E. H. LOOMIS.

An Absolute Determination of the E. M. F. of the Clark Cell. HENRY S. CARHART and KARL E. GUTHE.

The Resistance Offered by Iron Wires to Alternating Currents. ERNEST MERRITT.

On a Theorem of Clebsch's. PAUL SAUREL.

An Investigation of the Magnetic Qualities of Building Brick. O. A. GAGE and H. E. LAWRENCE.

Note: Robert Wilhelm Bunsen

New Books: *Hastings and Beach:* A Text-Book of General Physics; *William Watson:* Practical Physics; *Harper's Scientific Memoirs:* The Laws of Gases The Second Law of Thermodynamics; The Fundamental Laws of Electrolytic Conduction

Index

L. January, 1900.

TABLE OF CONTENTS

A Determination of the Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether. I. R. A. FESSENDEN.

Polarization and Internal Resistance of the Copper Voltameter. B. E. MOORE.

A New Method of Comparing Two Self-Inductances. H. V. CARPENTER.

New Books: *Wüllner:* Lehrbuch der Experimental Physik; *Ostwald's* Klassiker der Exakten Wissenschaften, Nos. 99 and 100; *Lengfeld:* Inorganic Chemical Preparations; *Hillyer:* Laboratory Manual.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

VOLUME X NUMBER 3

THE
PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS

ERNEST MERRITT, and FREDERICK BEDELL

LII

MARCH 1900

TABLE OF CONTENTS

On the Metabolism of Matter in the Living Body **EDWARD B. ROSA** 129
A Dissociation Theory of the Electric Arc **C. D. CHILD** 151
The Effect of Magnetization on the Elasticity of Rods. II **JAMES S. STEVENS** 161
The Influence of Temperature, Pressure, Used Solutions, and Size of Anodes on the Deposit of Silver Voltameters **J. F. MERRILL** 167
A Lecture Demonstration in Wave Motion **WILL. C. BAKER** 175
An Apparatus for the Determination of the Coefficient of Expansion of Air at Constant Pressure **C. F. ADAMS** 178
On the Change in Length of Soft Iron in an Alternating Magnetic Field 180 **LOUIS W. AUSTIN**
New Books: *Wentworth and Hill:* Text-book of Physics; *Franklin and Williamson:* Alternating Currents; *Aimé:* De l'influence de la pression sur les actions Chimiques; *Duhem:* Mechanique Chimique; *Miller:* American Telephone Practice 187

COPYRIGHT, 1899, BY THE MACMILLAN COMPANY

PUBLISHED FOR CORNELL UNIVERSITY
BY THE MACMILLAN COMPANY
LANCASTER, PA., AND NEW YORK
LONDON: MACMILLAN AND CO., LTD
BERLIN: MAYER AND MUELLER

THE PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, AND FREDERICK BEDELL

Two volumes of THE PHYSICAL REVIEW are published annually, these volumes beginning in July and January, respectively, and containing at least five numbers each. The price of subscription is two dollars and fifty cents a volume (five dollars a year), or fifty cents a number. Subscriptions should be sent to the publishers. THE MACMILLAN COMPANY, 41 *North Queen Street, Lancaster, Pa.*, or 66 *Fifth Avenue, New York;* Messrs. MACMILLAN & CO., LTD., *London;* or to Messrs. MAYER AND MUELLER, *Berlin.*

Previous to Volume V. (July–December, 1897) THE PHYSICAL REVIEW was published in annual volumes, each containing six bimonthly numbers, beginning with the July–August number, 1893. These may be obtained from the publishers at the former subscription price, three dollars per volume.

Subscribers who desire to have their copies of the REVIEW sent to them with the edges cut, can so obtain them by informing the publishers to that effect.

Correspondence relating to contributions should be addressed to the editors, at *Ithaca, New York.*

Manuscript intended for publication in THE PHYSICAL REVIEW must be communicated by the author; when publication in other journals is contemplated, notice to that effect should be given.

The authors of original articles published in the REVIEW will receive one hundred separate copies in covers, for which no charge will be made; additional copies, when ordered in advance may be obtained at cost.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

RICHARD MUELLER-URI

BRAUNSCHWEIG (Germany)

MANUFACTURER OF

Blown Glass for Scientific Purposes

ALL KINDS OF LECTURE APPARATUS AND SUPPLIES FOR CHEMICAL AND PHYSICAL LABORATORIES ❧ ❧

Chas. R. Cross's Vacua Scale—Röntgen Tubes—McFarlan Moore Apparatus—Elster & Geitel's Transformer without Oil Insulation and Tesla Series—Spectrum Tubes (Argon, Helium) with pure Gases or Solids—Electrical Tubes after Arons, Braun, Crookes, Ebert, Geissler, Goldstein, Hittorf, Lecher, Lenard, Puluj, Zehender—Radiometers—Photo-Electric Cells and Apparatus (Selenium, etc.), Electrometer Exner.—Atmospheric Electricity Apparatus.

—Apparatus for Gas-Analysis and Titration

Sole Maker and Furnisher of Patented Constructions:

115874	Röntgen-Tube for low tension currents.	58795	Perforator (Extractor).
112835	Current Demonstrating Apparatus.	38217	Sterilizor.

Best Chemical Glassware. Professor Weber's Glass and Jena Normal Glass

Catalogues and Special Prospectus on Desire

WM. GAERTNER & CO.

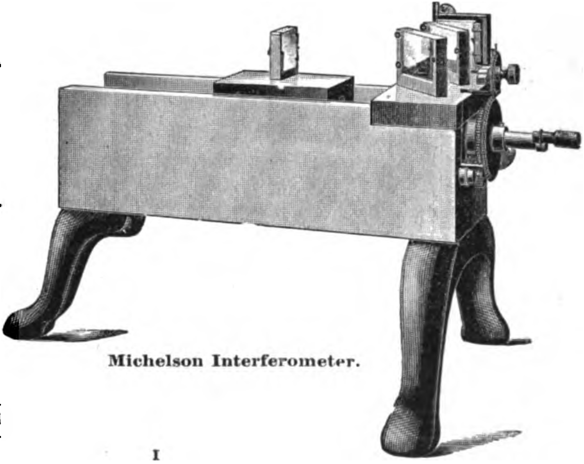
Astronomical & Physical Apparatus

152-158 E. 56th Street, CHICAGO

SPECIALTIES

- **Standard Apparatus of new and improved designs**
- **Astronomical Telescopes**
- **Spectroscopes**
- **Micrometers**
- **Michelson Interferometers**
- **Reading-microscopes and Telescopes**
- **Bolometers**
- **Heliostats**
- **Dividing Engines, Comparators**
- **General Laboratory Apparatus**
- **Universal Laboratory-supports**

Special attention paid to the construction of instruments for original investigation after our own or furnished designs.



Michelson Interferometer.

The A

AN INTE

GEORGE E.

Director of the Yerkes

The Le

FEBRU

On the Arc-Spectra
Platinum Group.

The Aberration of P

On the Causes of th

Radiation in a Magn

Minor Contributions

Reviews

MARC

The Concave Gratin
raphy
CHARLES LANE POOR a

On Certain New R
Phenomena Discov

Resumé of Solar Obs
Royal Observatory
during the Second

On the Arc-Spectra
Platinum Group. I

Minor Contributions

Reviews

Recent Publications

The

Published every mont
July and Septem

THE PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICA

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, AND FREDERICK BEDEL

Two volumes of THE PHYSICAL REVIEW are publi
these volumes beginning in July and January, respect
taining at least five numbers each. The price of
two dollars and fifty cents a volume (five dollars
cents a number. Subscriptions should be sent to
THE MACMILLAN COMPANY, 41 *North Queen Street*,
66 Fifth Avenue, New York; Messrs. MACMILL
London; or to Messrs. MAYER AND MUELLER, *Ber*

Previous to Volume V. (July–December, 18
REVIEW was published in annual volumes, each
monthly numbers, beginning with the July–Au
These may be obtained from the publishers at t
tion price, three dollars per volume.

Subscribers who desire to have their copies
to them with the edges cut, can so obtain the
publishers to that effect.

Correspondence relating to contributions s
to the editors, at *Ithaca, New York*.

Manuscript intended for publication in TH
must be communicated by the author; when
journals is contemplated, notice to that effect
The authors of original articles publishe
receive one hundred separate copies in
charge will be made; additional copies, whe
may be obtained at cost.

PUBLISHED FOR CORNELL UNI

THE MACMILLAN

ysical Journal

VIEW OF SPECTROSCOPY AND

ICAL PHYSICS

EDITED BY

AND

Numbers

IAR

BR

vn G

OF LEC
AL AN

Cross's Va
eitel's Tr
gon, Heli
okes, Ebe
ometers—
er.—Atm

Sole Make
Tube for
Demonstra

al Glassw

Ca

G

tron

152-1

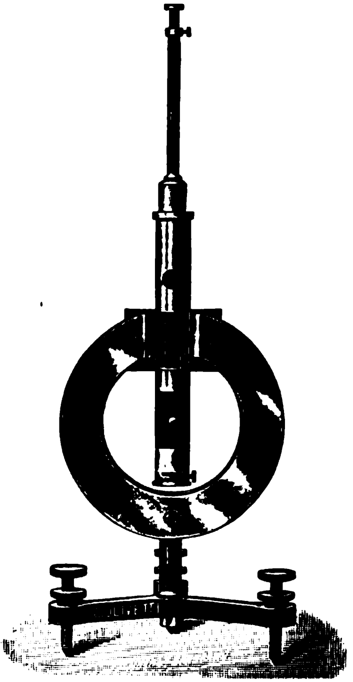
LTIES

s of new
s
copes

meters
es and

Comparat
y Apparat
ry-suppor

aid to the
ents for or
our own o



Professors and Assistants

in electrical laboratories don't like to give students fine galvanometers because such instruments are liable to be spoiled.

They are perfectly right

Students ⟶

don't like to work with cheaply made instruments, because they are not correct and are troublesome to handle.

They are perfectly right

Use **Olivetti's Reflecting Galvanometers**

and you will have high-grade up-to-date instruments which don't fear hard service.

Catalogues and prices for the asking

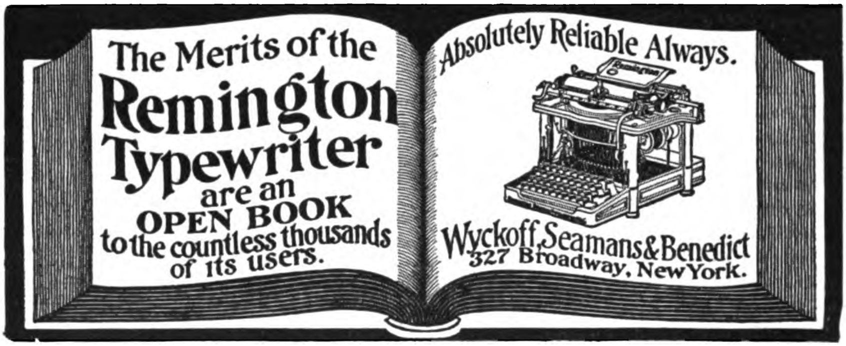
CAMILLO OLIVETTI
Maker of Electrical Instruments
IVREA, ITALY

The Principles of the Transformer

By FREDERICK BEDELL, Ph.D.
Assistant Professor of Physics in Cornell University

8vo. Cloth. Pp. ix + 416. Price, $3.25 *net*. 250 Illustrations

THE MACMILLAN COMPANY
NEW YORK: 66 FIFTH AVENUE
BOSTON CHICAGO SAN FRANCISCO



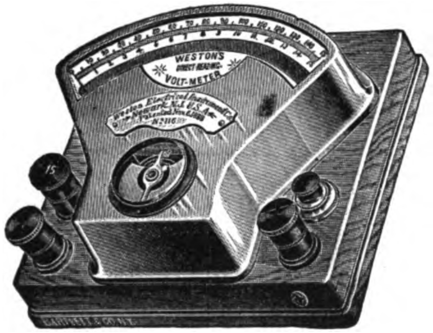
THE E. G. WYCKOFF CO., Sprague Bldg., Ithaca, New York.

WESTON

STANDARD
PORTABLE
DIRECT-READING

VOLTMETERS
MILLIVOLTMETERS
VOLTAMMETERS
AMMETERS
MILLAMMETERS

GROUND DETECTORS AND CIRCUIT TESTERS
WATTMETERS
OHMMETERS
PORTABLE GALVANOMETERS



WESTON STANDARD PORTABLE DIRECT-READING VOLTMETER FOR DIRECT CURRENT

Our Portable Instruments are recognized as The Standard the world over. The Semi-Portable Laboratory Standards are still better. Our Station Voltmeters and Ammeters are unsurpassed in point of extreme accuracy and lowest consumption of energy

Weston Electrical Instrument Co.

114-120 William St., NEWARK, N. J., U. S. A.

A History of Physics

In Its Elementary Branches, including the Evolution of Physical Laboratories

By FLORIAN CAJORI, Ph.D.

Professor of Physics in Colorado College.

Cloth Cr 8vo, $1.60 *net.*

This brief popular History gives in broad outline the development of the science of physics from antiquity to the present time. It contains also a more complete statemen. than is found elsewhere of the evolution of physical laboratories in Europe and Americat The book, while of interest to the general reader, is primarily intended for students and teachers of physics. The conviction is growing that, by a judicious introduction of historical matter, a science can be made more attractive. Moreover, the general view of the development of the human intellect which the history of a science affords is in itself stimulating and liberalizing.

"Prof. Cajori has followed his 'History of Mathematics' with a 'History of Physics' that is *even more interesting and valuable.* . . . Every teacher and student of physics should read it and add it to his library. It shows careful study, enthusiasm and a comprehensive grasp of the subject."—*Journal of Education.*

"A most valuable contribution to the subject."—*Scientific American.*

"Probably this will prove the most successful of all Prof. Cajori's histories."
—*The Nation.*

BY THE SAME AUTHOR

A History of Mathematics

Cloth 8vo, $3.50 *net.*

"What we have a right to expect in such a handbook is an agreeable narrative of the most material events in the history of mathematics, and this Professor Cajori incontestably supplies. The book was much wanted."—*The Nation.*

"A scholarship both wide and deep is manifest in this *History of Mathematics* which the author has infused with his own ardor in this department of science."
—*Journal of Education.*

"To the student with a love for mathematical science this book will be as entertaining as a romance."
—*The Transcript*, Boston.

A History of Elementary Mathematics

Cloth 12mo, $1.50 *net.*

"A most instructive, and at the same time a very readable piece of work, full of curious facts."—*The Bookman.*

"By no means an abridged edition of *The History of Mathematics* It is an entirely new book, giving a somewhat detailed account of the rise and progress of Arithmetic, Algebra and Geometry. The book should be read by all teachers of these subjects, and by mathematical students generally."
—*American Mathematical Monthly.*

"The product of wide and scholarly research . . . For its historical facts and its suggestiveness, this work should be read carefully by all students and teachers of mathematics."—*The Dial*, Chicago.

THE MACMILLAN COMPANY, Publishers, New York

The

General Electric Company's

Pocket Ammeters

INCLINED COIL POCKET AMMETER (Covered)

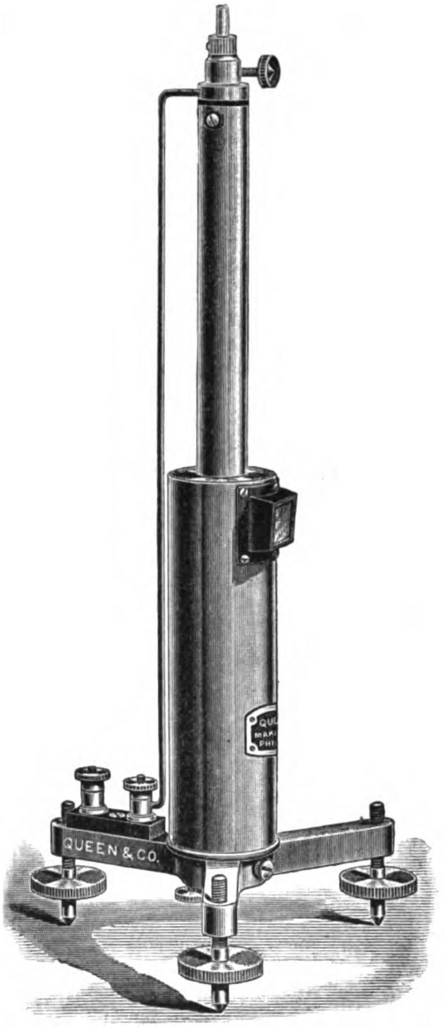
of twenty-five amperes' capacity can be used upon either direct or alternating current, and gives accurate readings for experimental work in the laboratory.

INCLINED COIL POCKET AMMETER (Uncovered)

This pocket ammeter is one of a complete set of portable inclined coil electrical measuring instruments, for use in the laboratory, central station and outside work.

GENERAL ELECTRIC COMPANY
Main Office: SCHENECTADY, N. Y.
Sales Offices in All Large Cities
For CANADA: Address the CANADIAN GENERAL ELECTRIC COMPANY, Toronto, Can.

QUEEN INSTRUMENTS



Queen D'Arsonval Galvanometer

In next issue we illustrate the Wall Pattern form. Send for special circulars on Photometers, Portable Testing Sets, Cable Testing Apparatus, X-Ray Apparatus.

POINTS WORTHY OF ATTENTION
- Ease of Levelling
- High Insulation
- Sensibility
- Extreme Simplicity

QUEEN & CO., INC.

J. G. GRAY, Pres.

Electrical and Scientific Instruments

59 Fifth Avenue
NEW YORK

1010 CHESTNUT ST., PHILADELPHIA.

Volume X. *March, 1900.* *Number 3.*

THE

PHYSICAL REVIEW.

ON THE METABOLISM OF MATTER IN THE LIVING BODY.

By Edward B. Rosa.

A GREAT many so-called metabolism experiments have been made upon the human body and upon animals in which a balance of nitrogen or of nitrogen and carbon was obtained. That is, the quantity of nitrogen or of nitrogen and carbon in the food was determined by analyzing samples of the food, and the quantity of these elements in the excreta and the products of respiration was also determined. If the nitrogen taken into the body during the course of an experiment exceeds that given off from the body in the same period, then there is a net gain of nitrogen, and this is taken to indicate an increase in the total amount of protein of the body. Conversely, if the outgo of nitrogen exceeds the receipts there is a net loss, and this indicates a loss of protein. As protein contains 16 per cent. of nitrogen, the quantity of protein gained or lost is found by multiplying the gain or loss of nitrogen by 6.25. But protein also contains 53 per cent. of carbon. Hence, before striking a balance of the carbon, we must insert on one side or the other of the account, the quantity of carbon in the protein which has been gained or lost. The net gain or loss of carbon is then taken as a measure of the amount of fat which has been gained or lost from the body during the experiment. As fat contains 76.5 per cent. of carbon, the quantity of fat gained or lost is found by dividing the gain or loss of carbon by the factor .765.

These experiments in which both a nitrogen and carbon balance are determined require a determination, as has been said, not only of the nitrogen and carbon of the food and excreta, but also of the quantity of carbon dioxid given off from the lungs and skin; that is, of the products of respiration. They are hence called respiration experiments. Sometimes the subject of the experiment has worn a mask in such a manner as to respire air drawn from a particular source, the composition of which is determined, while the air expelled from the lungs is carried away to a reservoir, and its contents of oxygen and carbon dioxid subsequently determined. This gives the quantity of oxygen absorbed and of carbon dioxid exhaled. But such an experiment neglects the respiration of the skin, and is unsatisfactory on other accounts. The better method is to confine the subject in a chamber and to analyze samples of the air supplied to the chamber, and also other samples drawn from the current of air flowing away from the chamber.

In the experiment reported by Atwater and Rosa in the September and October numbers of the PHYSICAL REVIEW, as well as in subsequent experiments with the same apparatus, a nitrogen and carbon balance was obtained and also a balance of energy. For the latter, the potential energy of the food eaten by the subject was determined by burning samples in a calorimeter; the potential energy of the excreta was determined by burning samples of the dried residue in a similar manner; the heat given off from the body during the experiment was also measured, the respiration chamber being at the same time a calorimeter, hence called a respiration calorimeter. A balance of energy can then be reached by taking account of the quantity of protein and fat gained or lost. Such a balance of energy, to within about one or two per cent. has been obtained in a considerable number of experiments at Middletown, and the work is still being carried on.

A balance of hydrogen is more difficult to obtain than are the nitrogen and carbon balances. This involves, of course, the determination of the organic hydrogen of food and excreta, and also the quantity of water in the same and the quantity of water exhaled from the skin and lungs. The water vapor contained in the current of air entering the calorimetric chamber, and also that leaving through the outgoing current of air are determined by absorption in sulphuric acid tubes at the same time that the carbon dioxid is absorbed by soda lime. But water being to some extent condensed upon the walls of the chamber, and absorbed by the clothing of the subject and the furnishings of the chamber, the exact determination of the water becomes a more difficult matter than that of the carbon dioxid, and a hydrogen balance has been seldom attempted.

It is impossible for one who has not been engaged in it to appreciate the difficulty and complexity of this kind of experimental work, or the enormous amount of labor and computation necessary to obtain the final results of a four days' respiration experiment. In the Middletown experiments analyses of samples of food were made in duplicate, and all determinations of carbon dioxid and water in the air current were in duplicate. But in spite of the greatest care residual errors are not inappreciable. And in weighing the food and drink, selecting samples for analysis, and in collecting and weighing the water condensed inside the chamber and outside in the freezers, errors of appreciable magnitude are bound to occur. Hence when one strikes a balance of nitrogen and carbon and computes the number of grams of protein and fat which have been gained or lost, the algebraic sum of a large number of errors appears in the result. Moreover, in an energy experiment this error is carried over into heat account in a magnified degree. Suppose, for example, that in the determination of the carbon of the food there is an error of one-half of one per cent.; the quantity of carbon in the daily ration being 261.5 g., this amounts to an error of 1.3 g., or 1.7 g. of fat. The heat value of a gram of fat being 9.4 large calories, 1.7 g. is equivalent to 16.0 calories, which is 0.7 per cent. of the total heat per day. An error in the nitrogen, however, produces a comparatively small error in the computed heat; for if the protein gained be reckoned too large, the fat gained will be too small in a corresponding degree, and the difference of heat value produced is relatively small.

In order to locate the sources of error and to reduce them in magnitude to a minimum, it is desirable to make use of every possible check upon the analyses and computations. Such a check may be found in the change of weight of the subject of the experiment, provided the oxygen taken up from the air by respiration can be determined. Direct determinations of oxygen have been made in short experiments, by analyzing the air in the chamber at the beginning and at the end, and admitting to the chamber during the experiment a known quantity of oxygen. But for long experiments this is impracticable, as the air of the chamber must be frequently renewed. On the other hand, it is impossible to analyze the air by sample, as is done for CO_2 and H_2O, and determine the percentage of oxygen in the ingoing and outcoming air currents with sufficient accuracy. For if 50 liters of air be admitted to the chamber per minute, the quantity of oxygen in 24 hours will be about 20,000 g. and an error of one-tenth of one per cent. in the determination would amount to 20 grams, which would be an error of three per cent. in the amount of oxygen absorbed by the lungs.

On the other hand, an oxygen balance may be obtained in a manner similar to that followed for N, C and H. If the quantity of these latter elements and the ash of the food and excreta are accurately determined, then the amount of oxygen becomes known. The oxygen of the water of income and outgo is accurately known when the quantity of water is determined, and hence one can readily calculate the total amount of oxygen taken into the body and the total given out. The difference will represent that derived from the atmosphere by respiration. If it be objected that this determination of oxygen by difference is not sufficiently exact it may be replied, first, that if the oxygen is not exact then the other elements, N, C and H, are not exact; and, second, that the dry residue of food and excreta contains over 30 per cent. of oxygen, a greater proportion than that of any other constituent except carbon, and hence a determination by difference is far more reliable than where that difference is a small quantity, as, for example, in the case of the protein and fat gained or lost. But the latter quantities are regularly calculated by difference, with no check upon the results to show them to be correct. It is now proposed in a similar manner to calculate the hydrogen and oxygen by difference, and then to determine the oxygen taken up from the air in the manner of Pettenkofer and Voit,[1] from the gain or loss of weight of the subject, and to compare the two results thus derived by two independent methods. Or, having computed in the manner already indicated the amount of oxygen absorbed from the atmosphere, we are to calculate the gain or loss of weight of the person or animal which is the subject of the experiment. And if this weight is ascertained by an accurate balance at the beginning and end of the experiment, we have a means of checking by the difference the accuracy of the experiment. But if the subject is weighed four times each day, that is at the beginning of each six-hour period, and the observed weight tallies each time closely with the computed weight, we have a still better verification of the work. The probability is that considerable discrepancies would appear between the observed and computed weights, which would point out the more important sources of error, and thus serve as a means of increasing the accuracy of the work, besides sometimes detecting accidental errors of importance which would not otherwise be noticed.

---

[1] The accuracy of weighing of these distinguished investigators was evidently not sufficient for the purpose.

It was such considerations as these that led to the purchase several years ago of an accurate scale, capable of weighing up to 100 kilos, which was put inside the calorimeter and the subject weighed himself several times during each experiment. However, this check was not applied, and the weighings, though made and recorded were never made use of. Having now retired completely from the investigation, I wish to present here the method which I devised about five years ago and to work it out for the experiment that was published in the recent numbers of the PHYSICAL REVIEW. The weighings were not as accurately nor as frequently made as they might have been; indeed, for this purpose they would better be made by the observers outside, and not by the subject within. I will indicate later how this can readily be done.

Table I, gives the balance of N,C,H,O and ash for the four days taken together. Under each element are two columns headed "In" and "Out," containing respectively the amounts of that element in the income of the body from all sources and the outgo of all kinds. These columns should balance, after the manner of the debit and credit columns of a ledger account, when one has inserted the balancing item. In the case of nitrogen this is the amount of nitrogen acquired from the protein of the tissues which was broken down

TABLE I.

Balance of Nitrogen, Carbon, Hydrogen and Oxygen.

Materials of Income and Outgo.		Total.	Nitrogen.		Carbon.		Hydrogen.		Water.		Oxygen.		Ash.	
			In.	Out.	In.	Out.	In.	Out.	In.	Out.	In.	Out.	In.	Out.
		Grams.												
Food, Water.	Income.	3684.0					412.0		3684.0		3272.0			
" Solids.		2208.0	76.4		1046.2		150.4				845.8		89.2	
Water of Drink.		5382.0					602.0		5382.0		4780.0			
Oxygen from Air.		2611.6									2611.6			
Feces, Water.	Outgo.	308.8						34.5		308.8		274.3		
" Solids.		115.2		5.0		53.4		7.3				30.5		19.0
Urine, Water.		6421.0						718.2		6421.0		5702.8		
" Solids.		281.4		73.7		50.3		13.4				68.0		76.0
Water of Respiration.		3765.0						421.1		3765.0		3343.9		
Carb. Dioxide of Res.		3279.8				894.5						2385.3		
From Comb. of Body Protein.		14.4	2.3		7.6		1.0				3.5			
" Storing of Body Fat.		72.7				55.6		8.7				8.4		
" Dimin. of Body Water.		338.1					37.8		338.1		300.3			
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.		150.4+1.0 −(7.3+13.4 +8.7)=122.0							1090.7				5.8	
			78.7	78.7	1053.8	1053.8	1203.2	1203.2	10494.8	10494.8	11813.2	11813.2	95.0	95.0

over and above what has been formed; for carbon, the amount contained in the fat stored away during the experiment; for hydrogen, the amount derived from the decrease of the water contained in the body; for oxygen, the amount derived from the atmosphere; and for ash, the loss of mineral matter of the body. These balancing items are printed in bold faced type.

The daily ration weighed 1,473 g., of which 921 g. was water and 552 g. dry solids. Thus in four days the water amounted to 3,684 g. and the dry solids to 2,208 g. These quantities are entered in the table in the column of totals. Taking the atomic weights of hydrogen and oxygen as 1 and 15.88, water contains 11.185% of hydrogen and 88.815% of oxygen. The water of the food, therefore, contains 412 g. of hydrogen and 3,272 g. of oxygen, and these items are entered in the proper columns under hydrogen and oxygen respectively. Analysis of samples showed that the dry residue contained 76.4 g. of nitrogen, 1,046.2 g. of carbon, 150.4 g. of hydrogen, and 89.2 g. of ash. The remainder, 845.8 g., is oxygen. All these items are entered in the table in their appropriate places. The 5,382 g. of water drunk (more than half of this was in coffee), in addition to that contained in the food, consisted of 602 g. of hydrogen and 4,780 g. of oxygen. The amount of water is also entered in the column of water. The fourth item of income, oxygen received from the air, is entered last of all, being a balancing item.

We get an approximate check upon the oxygen of the food in the following manner. The dry residue contained:

Protein, ...	478.4 g.,	of which	24.0%	is oxygen	= 114.8 g.
Fat, ...	276.0	" "	11.5%	"	= 31.6
Carbohydrates, ...	1364.4	" "	50.0%	"	= 682.2
Ash, ...	89.2	"			0.
Total	— 2208.0 g.			Total oxygen	— 826.6 g.

As these percentage compositions are not exact we cannot regard this as more than a rough check upon the direct analyses, although the true value may perhaps lie between the two.

The next four lines in the table give the amounts of the various elements in the solid and liquid excreta, the total weights of which are inserted in the column of totals, and the quantity of each element in the "out" column of that element. The water and carbon dioxid of respiration in like manner go into the column of output, the latter being exactly $\frac{3}{11}$ carbon and $\frac{8}{11}$ oxygen; that is, the atomic weight of carbon is 11.91 if oxygen is 15.88, or 12 if oxygen be taken at 16.

We now come to the balancing items:

First, the output of nitrogen is 2.3 g. more than the income. We therefore insert 2.3 as the balance, and this multiplied by 6.25 gives 14.4 g. as the weight of protein lost. This is entered in the column of totals and 7.6 g. of carbon, 1 g. of hydrogen and 3.5 g. of oxygen also contained in the protein are charged into the debit side of those accounts.

Second, the receipts of carbon exceed the output by 55.6 g. Hence we insert this as the balancing item and put 72.7 g. in the column of totals as the weight of the fat in which this carbon is stored up; 8.7 g. of hydrogen and 8.4 g. of oxygen are also contained in this fat, and these items are accordingly entered in their proper places.

Third, the output of hydrogen exceeds the receipts by 37.8 g. This is explained by the fact that the amount of water in the body must have decreased during the experiment by 338.1 g., for no other source of hydrogen remains to be considered. This is indeed the only way of ascertaining the change in the content of water in the body. The accuracy of this determination of course depends upon the accuracy of the several items occurring in the hydrogen columns. There is reason to believe they are less accurate than those of the nitrogen and carbon accounts, but the result of this computation is to give a check which tests the hydrogen account more than any other in the table.

Fourth, the item of 338.1 g. of water lost must be entered in the debit column of the water account, and 300.3 g. in the oxygen account. It represents the amount derived from the body's capital stock at the beginning of the experiment. To balance the water account we must take note of the fact that the organic hydrogen of the food when it is oxidized forms water, which must be charged into the debit side of the water account; or rather, the excess of hydrogen contained in the food (150.4) and that contained in the protein used up (1.0), over that contained in the feces (7.3), urine (13.4) and that stored up in the fat accumulated (8.7). Thus: 150.4 + 1 − (7.3 + 13.4 + 8.7) = 122.0 g. of hydrogen, which has been oxidized and formed 1090.7 g. of water. Entering this item on the debit side of the water account, the latter balances exactly.

Fifth, the oxygen account is now complete except the balancing item of 2611.6 g., which represents the quantity of oxygen derived from the atmosphere. This assumes that whatever quantity of oxygen is stored up as such or as oxyhaemoglobin in the blood and in the tissues does not vary by an appreciable amount; or, at least, that it was sensibly the same at 7 o'clock on the morning that the experiment began that it was at 7 o'clock on the last morning. This shows an absorption of 27.2 g. per hour or .45 g. per minute.

The balance of oxygen may be put in another form by omitting from both sides of the account the oxygen from the water which is received into the body. It then appears as follows:

BALANCE OF OXYGEN FOR FOUR DAYS.

	Receipts.	Output.
From the food	845.8	
From 14.4 g. protein of the body	3.5	
Excreted in feces (solids)		30.5
" urine "		68.0
" carbon dioxid		2385.3
" water from organic hydrogen		968.7
Stored away in fat		8.4
Balance from atmosphere	2611.6	
	3460.9	3460.9

Sixth, the column of ash does not balance as closely as it ought. There could not be a loss of 5.8 g. of mineral matter, and hence this difference must be mainly due to error in some one or more of the items given. Indeed, the record does not show positively whether all three items were actually determined. A change in any of them would alter the oxygen item by the same amount, but this would affect the final result only slightly, and for the present purpose it makes no difference.

We are now in position to calculate the loss of weight of the person under investigation, during the four days he was confined in the respiration chamber. We must first note the fact that the feces passed during these four days weighed 498.4 g., being 74.4 g. more than those which belonged to that period, the latter being distinguished by the use of charcoal.

BALANCE OF MATTER.

Income.	Grams.	Outgo.	Grams.
Food	5892.0	Feces	498.4
Drink	5382.0	Urine	6702.4
Oxygen from air	2611.6	Water of respiration	3765.0
Balance = Loss of weight	360.0	Carbon dioxid of respiration	3279.8
	14245.6		14245.6

This loss of 360 grams in weight is made up as follows:

Loss of water from body, see Table I., =	338.1 g.
" " feces " "	74.4
" " protein " " " " " =	14.4
" " ash " " " " "	5.8
	432.7
Gain of fat, " " "	72.7
Net loss of weight, as above,	360.0

Now the weight of the man at the beginning was 68,420 g. and at the end was 68,000[1] g., making a loss of 420 g., or 60 g. more than the calculated loss. This is of course not a very close agreement. The discrepancy may be partly accounted for by the fact the weighings of the subject by himself were not as exact as if made by some one else, and were not made at the exact time of beginning and end. The computed loss of weight is of course in error by an appreciable amount, but for want of more reliable weighings we cannot say definitely what the error is.

Tables II. and III. give the results of a similar computation for each day separately. The food was the same each day, although there was a slight variation of the water of the food and on the first day the water of the drink was about 200 g. more than on the subsequent days. The feces, although passed in only two portions, were assumed to be separated in equal quantity each day. There was only a slight variation from day to day in the evolution of

---

[1] The record of this weight is 67,200, but from a previous weight it is evident that this is an error and should be 68,000.

TABLE II.

Balance of Nitrogen, Carbon and Hydrogen.—By Days.

Materials of Metabolism.		**Nitrogen.**							
		1st Day.		**2d Day.**		**3d Day.**		**4th Day.**	
Food, Water.	Income.								
" Solids.		19.1		19.1		19.1		19.1	
Water of Drink.									
Oxygen from Air.									
Feces, Water.	Outgo.								
" Solids			1.2		1.3		1.2		1.3
Urine, Water.									
" Solids.			18.8		18.7		18.3		17.90
Water of Respiration.									
Carb. Dioxide of Res.									
From Comb. of Body Protein.		0.9		0.9		0.4		0.1	
" Storing of Body Fat.									
" Dimin. of Body Water.									
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		20.0	20.0	20.	20.	19.5	19.5	19.2	19.2
Protein Consumed:		5.65		5.65		2.5		0.6	

Total = 14.4

Materials of Metabolism.		**Carbon.**							
		1st Day.		**2d Day.**		**3d Day.**		**4th Day.**	
Food, Water.	Income.								
" Solids.		261.5		261.5		261.6		261.6	
Water of Drink.									
Oxygen from Air.									
Feces, Water.	Outgo.								
" Solids.			13.3		13.3		13.4		13.4
Urine, Water.									
" Solids.			12.8		12.8		12.5		12.2
Water of Respiration.									
Carb. Dioxide of Res.			226.7		221.8		220.7		225.3
From Comb. of Body Protein.		3.0		3.0		1.3		0.3	
" Storing of Body Fat.			11.7		16.6		16.3		11.0
" Dimin. of Body Water.									
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		264.5	264.5	264.5	264.5	262.9	262.9	261.9	261.9
Fat Stored:		1.52		21.7		21.4		14.4	

Total = 72.7

TABLE II.—*Continued.*

Materials of Metabolism.		Hydrogen.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	103.0		102.8		103.0		103.2	
" Solids.		37.6		37.6		37.6		37.6	
Water of Drink.		167.0		144.3		145.4		145.3	
Oxygen from Air.									
Feces, Water.	Outgo.		8.6		8.6		8.7		8.6
" Solids.			1.9		1.8		1.8		1.8
Urine, Water.			199.5		213.1		161.3		144.3
" Solids.			3.4		3.4		3.3		3.3
Water of Respiration.			107.1		99.7		101.3		113.0
Carb. Dioxide of Res.									
From Comb. of Body Protein.		0.4		0.4		0.2		0.0	
" Storing of Body Fat.			1.8		2.6		2.6		1.7
" Dimin. of Body Water.		**14.3**		**44.1**			**7.2**		**13.4**
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		322.3	322.3	329.2	329.2	286.2	286.2	286.1	286.1

TABLE III.

Balance of Water, Oxygen and Ash.—By Days.

Materials of Metabolism.		Water.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	921.0		919.0		921.0		923.0	
" Solids.									
Water of Drink.		1492.8		1289.9		1299.5		1299.8	
Oxygen from Air.									
Feces, Water.	Outgo.		77.2		77.2		77.2		77.2
" Solids.									
Urine, Water.			1783.6		1906.0		1440.9		1290.5
" Solids.									
Water of Respiration.			957.9		891.7		905.9		1009.5
Carb. Dioxide of Res.									
From Comb. of Body Pro.									
" Storing of Body Fat.									
" Dim. of Body Water.		**127.8**		**396.0**			**65.6**		**120.1**
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.		277.1		270.0		269.1		274.5	
Loss of Ash.									
		2818.7	2818.7	2874.9	2874.9	2489.6	2489.6	2497.3	2497.

TABLE III.—*Continued.*

Materials of Metabolism.		**Oxygen.**							
		1st Day.		**2d Day.**		**3d Day.**		**4th Day.**	
Food, Water.	Income.	818.0		816.2		818.0		819.8	
" Solids.		211.5		211.5		211.4		211.4	
Water of Drink.		1325.8		1145.6		1154.1		1154.5	
Oxygen from Air.		664.3		645.9		642.6		658.8	
Feces, Water.	Outgo.		68.6		68.6		68.6		68.5
" Solids.			7.6		7.6		7.6		7.7
Urine, Water.			1584.1		1692.9		1279.6		1146.2
" Solids.			17.3		17.3		16.8		16.6
Water of Respiration.			850.8		792.0		804.6		896.5
Carb. Dioxide of Res.			604.4		591.6		588.6		600.7
From Com. of Body Pro.		1.4		1.4		0.6		0.1	
" Storing of Body Fat.			1.7		2.5		2.5		1.7
" Dim. of Body Water.		113.5		351.9			58.4		106.7
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.									
Loss of Ash.									
		3134.5	3134.5	3172.5	3172.5	2826.7	2826.7	2844.6	2844.6

Materials of Metabolism.		**Ash.**							
		1st Day.		**2d Day.**		**3d Day.**		**4th Day.**	
Food, Water.	Income.								
" Solids.		22.3		22.3		22.3		22.3	
Water of Drink.									
Oxygen from Air.									
Feces, Water.	Outgo.								
" Solids.			4.8		4.8		4.7		4.7
Urine, Water.									
" Solids.			19.4		19.4		18.8		18.4
Water of Respiration.									
Carb. Dioxide of Res.									
From Comb. of Body Pro.									
" Storing of Body Fat.									
" Dim. of Body Water.									
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.									
Loss of Ash.		1.9		1.9		1.2		0.8	
		24.1	24.1	24.1	24.1	23.6	23.6	23.2	23.2

carbon dioxid, but a greater difference in the water of respiration and a still greater variation in the urine. The quantity of the latter was so great the first two days that the total amount of water in the body decreased 523.8 g., but during the last two days it increased 185.7 g. The quantity of oxygen derived from the air varies but little, and in nearly the same degree as the evolution of carbon dioxid. The quantity of both oxygen and carbon dioxid was greater on the first and fourth days than on the two intervening days. The ratio of the carbon dioxid exhaled to the oxygen absorbed, called the respiratory quotient, is so important that I have put down the value for each day.

	1st Day.	2d Day.	3d Day.	4th Day.		Total.
Carbon dioxid exhaled,	831.1	813.4	809.3	826.0		3279.9
Oxygen absorbed,	664.3	645.9	642.6	658.8		2611.6
Respiratory quotient,	1.251	1.259	1.259	1.254	Ave.,	1.256

The respiratory quotient for the first day is 0.4% less than the average, corresponding to 2.6 g. of oxygen: that of the second and third days is 0.2% more, corresponding to 1.6 g. of oxygen, while that of the fourth day differs still less, corresponding to only 1.1 g. of oxygen. We know that the respiratory quotient should not be exactly constant from day to day in this experiment, because the receipts and expenditures of oxygen aside from respiration varied slightly. But these variations were only slight, and the nearly constant value of the respiratory quotient is an evidence of the correctness of the values of the oxygen absorbed from the atmosphere, as calculated in the table.

Table IV. gives the balance of matter for each day of the experiment and the calculated gain or loss each day. For the first day there is a discrepancy of 145.4 g. This indicates either a serious error in weighing or else a large error in the water account of that day. For the second day the difference is — 42.6 g., the third day practically nothing, the fourth day — 42.1 g., the total for the four days being 60 g. If the weighings had been more accurate and at exactly 7 o'clock each morning, the result would be more conclusive. But the present purpose is to show how this check may be made in future, rather than to seek to confer any benefits upon past experiments.

TABLE IV.

Balance of Matter.

		1st Day.		2d Day.		3d Day.		4th Day.	
Income.	Food, Water.	921.0		919.0		921.0		923.0	
	" Solids.	552.0		552.0		552.0		552.0	
	Drink, Water.	1492.8		1289.9		1299.5		1299.8	
	Oxygen of Air.	664.3		645.9		642.6		658.8	
Outgo.	Feces.		—		216.6		—		281.8
	Urine.		1855.3		1977.6		1510.6		1358.9
	Water of Respiration.		957.9		891.7		905.9		1009.5
	CO_2 of Respiration.		831.1		813.4		809.3		826.0
	Balance.		14.2		492.5		189.3		42.6
		3644.3–	3644.3	3899.3–	3899.3	3415.1–	3415.1	3476.2–	3476.2
	By Scales.	Lost	160.0	Lost	450.0	Gained	190.0	Lost	0.0
	By calculation above.	"	14.6	"	492.6	"	189.3	"	42.1
	Difference.		+145.4		−42.6		−0.7		−42.1

Difference for four days 60 grams, lost more by scales than by calculation.

But one can also calculate the weight of the subject at other times of the day and additional checks may thus be instituted which might be of service in locating errors and deriving the largest possible amount of information from an experiment. Suppose that it is desired to calculate the weight of the subject at the beginning of each 6-hour period, that is at 7 A. M., 1 P. M., 7 P. M. and 1 A. M. of each day. The CO_2 and H_2O of respiration are separately determined for each such period any way, and it is only a matter of slight computation to get all the data for a check upon the work four times each day, having the weight of the subject by an accurate scale at the same times.

In computing the fat gained or lost from the gain or loss of carbon, it is assumed that the glycogen and other carbohydrates of the body at 7 A. M. each day are constant in quantity, the diet being uniform from day to day. In order that the same assumption might hold true, or approximately so, for the six-hour periods it would be desirable to serve the meals just after the beginning of the periods. Then the glycogen content of the liver and tissues would be more nearly uniform than if supper were served at 6:30, as was done in these experiments. Even then, of course, it would vary, but the error introduced by the probable variation would not be considerable.

TABLE V.

Table of Gain and Loss.

	1st Day.		2d Day.		3d Day.		4th Day	
	Gain.	**Loss.**	**Gain.**	**Loss.**	**Gain.**	**Loss.**	**Gain.**	**Loss.**
Water.		127.8		396.0	65.6		120.1	
Feces.	106.0			110.6	106.0			175.8
Protein.		5.7		5.7		2.5		0.5
Fat.	15.2		21.7		21.4		14.4	
Ash.		1.9		1.9		1.2		0.8
Balance.	**14.2**		**492.5**			**189.3**	**42.6**	
	135.4	135.4	514.2	514.2	193.0	193.0	177.1	177.1

In the accompanying figures a graphic representation of the change of weight of the person during the 96 hours of the experiment is given. The points representing the weights at the end of every 6 hour period are inclosed in small circles, and the weight as computed is given in figures. Between these points the values are not exact, for it has been assumed that the evolution of H_2O and CO_2 and the absorption of oxygen were uniform during each period. Of course these quantities are greater during the exercise incident to dressing and eating than during subsequent rest, but for the purpose of illustrating the fluctuation of weight during a day, or during four successive days, this variation may be neglected. It would be of very great interest if we had the precise weight of the subject at each of these 17 six hour stations. Another source of uncertainty in the calculated weights at the six hour stations is that the respiratory quotient, determined above for each day separately, was assumed to be constant for the different periods of each day. But in experiments carried out with that in view it might be separately computed for each period. That might give information of value in itself as showing to what extent oxygen may be stored up in the body.

The chart is mainly self-explanatory. Starting at 7 o'clock on the morning of the first day with a weight of 68,420 g. the subject loses 45 g. in one hour by respiration. That is, the loss of CO_2

and H_2O above the gain of oxygen by respiration is 45 g. per hour during the first 6 hours. Breakfast adds 675 g. to his weight.

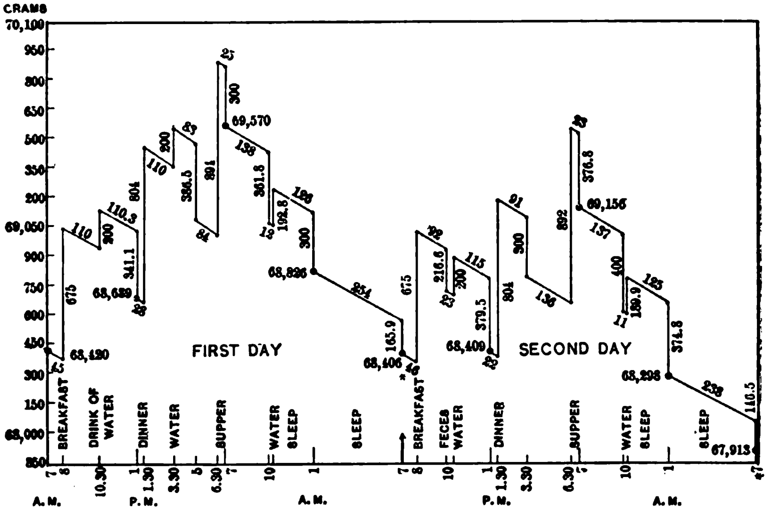


Fig. 1.

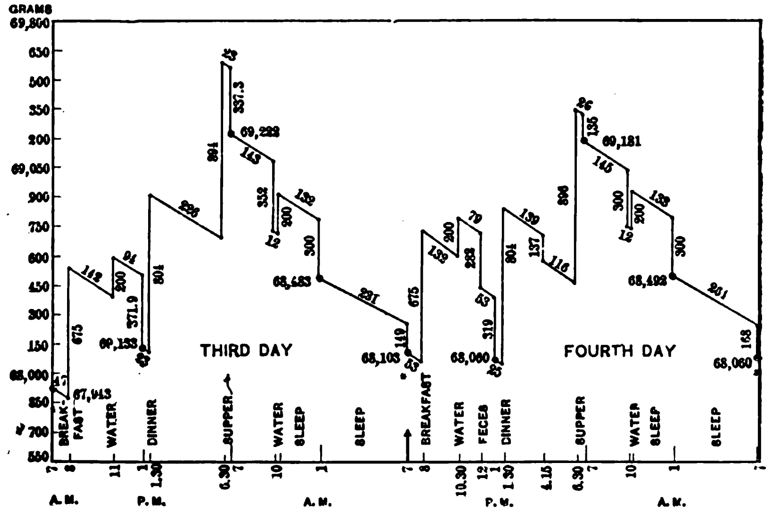


Fig. 2.

Of course the gain of weight due to breakfast was not instantaneous, but it was not deemed necessary or desirable to indicate the time occupied in eating meals by a sloping line. During the next 2½ hours until 10:30 a loss of 110 g. occurred by respiration, and then a drink of water increased the weight by 200 g. A further loss of 110.3 g. by respiration was followed by a decrease due to passage of urine, and at one o'clock the weight should have been 68,689 g. We have no observed weight to compare with it, but the weights we have at seven o'clock each morning are plotted in the chart as stars.

The charts show very clearly the considerable fluctuations of weight throughout a period of 24 hours. The minimum weight is at eight o'clock, just before breakfast, and the maximum just after supper. The slope of the lines showing the loss of weight by respiration is nearly uniform, as the loss of weight is nearly as rapid at night during sleep as during the day. The evolution of CO_2 is only slightly more than half as much at night as during the day (and in a work experiment only about one-fourth) but the absorption of oxygen is also less in (nearly) the same ratio. On the other hand, the rate at which water is given off from the skin and lungs is nearly as great at night as in the day in a rest experiment (although far less at night than during the day when at work). Hence while there was a loss of 265 g. by respiration in 6 hours, from 7 a. m. to 1 p. m. on the first day, there was a loss of 254 g. from 1 a. m. to 7 a. m. during sleep.

While the check that is thus afforded by knowing the weight of the subject at intervals during the experiment would not detect small errors in the determination of nitrogen and carbon, it would detect appreciable errors in the determination of the water, and this is the quantity most in need of such a check. Considering the great labor and cost of complete respiration experiments it appears of the greatest importance to use every possible means to reduce errors to a minimum and to derive the maximum amount of information from each experiment.

In order to weigh the subject with the greatest possible accuracy, he should be seated quietly on the platform of the balance, and the weighing done from without. This may be accomplished by using a rider, controlling it by a rod from outside, in a manner similar to that in a chemical balance. The subject would set the heavy weight at such a point that the beam nearly balanced. The exact balance would then be secured by the rider. The beam should be visible from without through the window, so that readings of the weight and rider could be made by the observer outside. The subject ought, however, to note and report the reading, when the weighing is finished, as a check upon the record of the observer. The position of exact balance could be obtained by use of a reading telescope, set in the walls of the chamber and a graduated scale attached to the beam would swing to and fro in the field. By observing the range, very close settings could be made, and with a good Fairbanks balance readings could be made at least to a single gram.

In order that these close readings be as reliable as possible, several successive weighings should be made. The subject is constantly growing lighter by the losses due to respiration. Hence the rider could be set a little to one side of a balance, and the time noted when exact balance was secured. A second setting would give a balance, say about a minute later. Let five such weights be made, extending before and after the time at which the weight is desired, and from the mean the true weight could be obtained to within a gram at least, not counting any constant error due to inaccuracy of the balance, which would be of no moment in taking the difference.

The subject should have a light suit of clothes, without pockets, and be weighed with his clothes on. Any moisture contained in his clothing would then be reckoned as part of himself. In a rest experiment this would be small, and the disturbance due to the exercise of dressing and undressing would be avoided.

Mechanical Work.

The bicycle-dynamo combination, which we employed to develop and measure mechanical energy at the expense of the potential energy of food and tissue was not the ideal form of ergometer. It was easily arranged and served our purpose for the time being. But it required calibration and the energy expended in friction was somewhat variable; moreover, it is probably not a machine in which the maximum efficiency of conversion could be effected. A better arrangement would be a simple treadmill. That is, a movable inclined plane consisting of slats attached to two endless chains, the latter running over sprocket pulleys. A fly wheel to steady the motion might consist of a heavy copper disc revolving between the poles of permanent magnets, thus serving also as a brake. By varying the position of the magnets the braking effect and, therefore, also the rapidity of motion could be varied, and by varying the inclination of the plane greater or less force would be exerted. The three quantities required in order to calculate the work done in any given time would be (1) the angle of elevation of the plane, (2) the whole number of revolutions of the flywheel or sprocket pulley and the distance traveled by the plane for each such revolution, and (3) the weight of the person operating the mill. The first is easily determined by reading a graduated sector, the base being level ; the second would be recorded by a simple counter; the third should be frequently determined for purposes of a check upon the observations and analyses, and hence would not involve any additional labor. This form of ergometer would enable a large or small amount of work to be done, would give a high efficiency of conversion, would require no calibration, would involve no error due to varying friction, would require no record of varying speed to be made, and could be constructed at a reasonable cost. With it a large variety of experiments could be made.

The conversion of the energy of food and tissue into mechanical energy is a profound mystery. We cannot understand how a contracting muscle transfers the energy liberated by the oxidation of matter into an outside portion of matter. Perhaps the heat produced by the oxidation is transformed by some process equivalent to that of a heat engine. If the second law of thermodynamics does govern the process, it may be that the requisite difference of temperature occurs only in spaces of the order of magnitude of molecules, and hence cannot be measured experimentally. Or it may be that the oxidation is accompanied by an evolution of less heat when work is done, as is the case when zinc is consumed in a battery furnishing a current of electricity. In other words, that it is in some way electrical, and hence that no such difference of temperature exists as would be required in a heat engine or its equivalent. Some such theory has been put forward, although it is far from explaining the process. It is possible that further experiments such as can be carried on with the respiration calorimeter will give such additional information as at least to assist in formulating a more satisfactory theory of the process.

But there is another case of interest which demands experimental examination. That is the converse case where work is done upon a man, for example, by an outside agent. If a laborer spends a day in hoisting brick to the top of a building by means of a windlass, the heat which his body gives out plus the work done upon the brick will equal the net potential energy of food and body tissue used up in the same time. Now suppose that next day he lowers the same brick to the ground again. The work done is negative in the mechanical sense, although the force exerted in handling the windlass is nearly as great as in hauling them up. Will the heat evolved *minus* the loss of potential energy of the brick equal the potential energy of food and body tissue used up? And will the heat yielded by the body be more or less than on the preceding day?

Or, again, suppose the windlass is driven by an engine while the laborer grasping the handle holds back with all his might. The engine does a large amount of work upon the man. What is the form that this energy assumes? Is oxidation checked by the exertion of force upon muscle?

Or, again, suppose a man is walking down hill. If he weighs 150 pounds and walks 10 miles down a grade of 14½° he will descend vertically 2½ miles and the potential energy given up by his body as a whole will be 1,980,000 foot pounds, which is equivalent to a horse-power working for an hour. What form does this energy take? Such an experiment could be performed in a respiration calorimeter by using the proposed treadmill for an inclined plane, and driving it by an electric motor, having the man walk down hill as the plane was driven up. Perhaps such experiments have already been made, but so far as I am aware no satisfactory answers to these questions have been reached.

An intermediate case is where force is exerted without doing work, as when a weight is held in the hands without being raised or lowered.

The suggestion that there is an energy-bearing product of mental effort which is capable of being apprehended by physical instruments is contrary to our physical notions. The increased generation of heat in the brain and head, due to increased circulation of the blood or increased oxidation of tissue or other oxidizable material during mental activity is however something that can be studied. But it would require special apparatus other than, or in addition to, the respiration calorimeter.

WESLEYAN UNIVERSITY, MIDDLETOWN, CONN., September 5, 1899.

A DISSOCIATION THEORY OF THE ELECTRIC ARC.

By C. D. Child.

A SERIES of very instructive photographs of the alternating current arc were shown by Mr. N. H. Brown in the Physical Review for November, 1898. An experiment recently performed by the present writer bears somewhat upon some of the phenomena there noticed and it may not be amiss to review briefly the results of Mr. Brown's article before describing this experiment. In order to refer more easily to the photographs Plate II. of that article is here reproduced. These are photographs of the alternating current arc taken upon a rapidly moving film. A screen with a narrow slit was so placed that light from but a small section taken lengthwise of the arc fell on the film. The films moved from left to right, and accordingly when read from right to left, they give the history of the arc through successive alternations. There appear to be two light fronts, one starting somewhat sooner than the other. As indicated by Mr. Brown, it is the negative one which starts the sooner, but moves with the slower velocity. The different photographs were taken with different velocities of the film and with different distances between the carbons but the general characteristics are the same in all here reproduced.

There would seem to be no doubt as to the cause of the fact that the arc starts at the negative carbon. As was shown the carbon which is negative at the beginning of a passage of the current was in the previous discharge positive, and it is well known that after the current has once been established the positive carbon is the hotter. This carbon would continue to be the hotter after the previous discharge had ceased, the vapors in its immediate vicinity would continue to be hotter than elsewhere and the current would accordingly begin at this point.

The explanation of the fact that the positive light front appears to move the more rapidly is not so simple. But though not simple, it may be the means of giving us a much better knowledge of the arc than that which we now possess. The explanation was suggested that the current was at least in part carried by ions and that the positive ions move with the greater velocity. Mr. Brown does not appear to consider this a probable explanation, and in fact it does not seem to be a possible one in the form suggested, *i. e.*, that the greater velocity of the positive ions may be due to the greater fall of potential at the positive carbon. For the distance through which the positive ions have the greater velocity is shown by the photographs to be greater than the space in which the fall of potential is so great and millions of times greater than the mean free path of the molecules at atmospheric pressure. We cannot see how the mean free path of the ions carrying the current can be greater than that of ordinary molecules. The ions will not maintain their velocity more than the distance of their mean free path beyond the space where the fall of potential is so great, unless the air move as one mass, but the photographs show that there is a motion from the negative to the positive as well as from the positive to the negative, and the air, as a whole, cannot move in both directions at once.

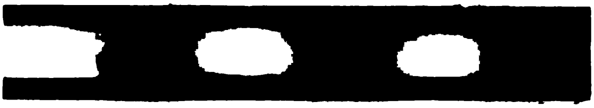
There is, of course, the possibility that the effects shown in the photographs are not caused by ions of molecular size, but particles of carbon which are driven off. But if this were the case, we should expect a disturbance of some kind at the point where the streams of negative and positive particles meet, and that this would be shown in the photographs. Nothing of this sort, however, is noticeable.

There are indications in the photographs of a phenomenon which would afford a still stronger reason against the idea that the greater velocity is caused by the greater fall of potential at the positive carbon. In one or two cases, especially in the discharge shown at the left hand of 2*A* the negative appears to move slower at the same time and in the same region where the positive front is moving more rapidly. If this should be shown to be the case, it would afford very strong evidence that the current is carried by ions, and that the positive ones move the more rapidly, not because of the greater fall of potential at the positive carbon, but because they do so even with the same potential gradient.

It is not known to the present writer whether it is possible to

PHYSICAL REVIEW, LII.
(To face page 152.)

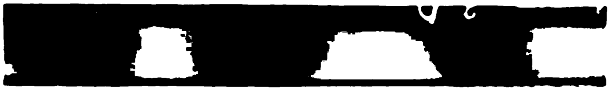
PLATE II.



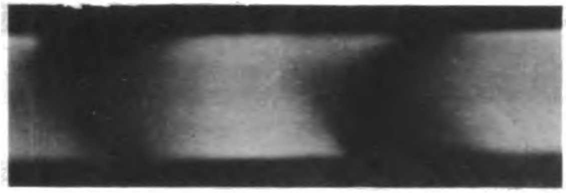
2A

2B

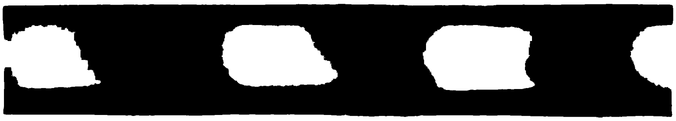
2C



2D



2E



2F

N. H. BROWN. PHOTOGRAPHIC STUDY OF THE ELECTRIC ARC.

[To accompany an article by C. D. CHILD.]

make photographs of such a character as to decide definitely this point, and in the absence of such knowledge it seemed well to consider the conclusions to which such a hypothesis would lead. Moreover the experiment to which reference has been made tends to substantiate this view. The following article will accordingly be devoted, first, to a discussion of the hypothesis that the current is carried in the arc by ions and that the positive ions with the same potential gradient move the more rapidly; and second, to a description of the experimental work performed.

As is well known, this resistance of the arc is not an ohmic resistance. A minimum difference of potential is necessary in order to maintain an arc, and when the current is increased the apparent resistance becomes smaller. Indeed, the resistance decreases so rapidly in the case of poles of solid carbons with a definite distance between them that the difference of potential becomes smaller as the current becomes greater.[1] The greater part of the fall of potential through the arc occurs at the surface of contact between the conducting gas and the carbons, and by far the greater fall is at the positive pole. Diagram 1 represents the fall of potential between two poles according to the data found by Mrs. Ayrton.[2] The continuous line would represent the case when a current of 10 ampères flows between the solid carbons at a distance from each other of 8 cm. The explanation often offered is that there is a counter E. M. F. in the arc caused by a combination of the Thomson and Peltier effects. However, it has never been possible to find any trace of the counter E. M. F. after the impressed E. M. F. has been removed. This is surprising since the gases of the arc can be shown to be conducting for an appreciable time after the current has ceased to flow, and a counter E. M. F. acting for 1/200 sec. would surely be detected.[3] If the effect is a true Thomson effect, it will last as long as the inequalities of temperature continue. That the inequalities do last for an appreciable time is shown by photographs of the arc. There may, of course, be some action similar to the Thomson effect but in just what the similarity consists has not yet been shown.

[1] Lond. Elec., Vol. 34, p. 366.
[2] Lond. Elec., Vol. 41, p. 720.
[3] Lond. Elec., Vol. 39, p. 615.

On the other hand the suggestion that the current in the arc is carried by ions and that the positive ions have the greater velocity would appear to offer a satisfactory explanation of the arc. A similar hypothesis was given by Prof. Zeleny to account for the phenomena in the case of discharge produced by X-rays.[4] His hypothesis needs but little modification to adapt it to the case in hand. The principal difference would be that in the case described by Prof. Zeleny the negative ions had the greater velocity, while in the case in hand the positive ions have the greater velocity.

We must suppose that the intense heat of the arc dissociates the gases of the arc into positive and negative ions. We cannot at present know whether the charges on the oppositely charged ions are or are not equal. But in any case it would not alter the essential idea of this article, and for simplicity we may assume an equal number of positive and negative ions with equal charges on the two kinds of ions. On being dissociated the positive ions move toward the negative pole, and the negative ions towards the positive pole. No doubt recombination and again dissociation take place frequently. But however often this may occur there will be an action equivalent to a motion of the negative ions in one direction and the positive in the opposite direction.

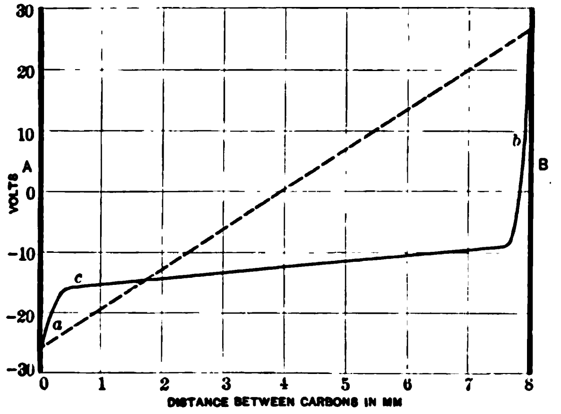


Fig. 1.

There would be an excess of positive ions in the vicinity of the negative pole and of the negative ions in the vicinity of the positive pole somewhat as indicated in Fig. 2, where the negative pole is at the left. The amount of electrification is shown by the ordinates. This electrification would produce an effect on the potential. Without any electrification the potential would fall regularly from one pole to the other as shown by the dotted line in Fig. 1. The electrification would change it so as to conform roughly with the continuous line in Fig. 1.

---

[4] Phil. Mag. (5), Vol. 46, p. 120.

If now the positive ions move with the greater velocity, they would in general take less time in reaching the pole toward which they are moving, and there would consequently be fewer of them between the poles at any instance of time. In a similar way if two armies having equal numbers of soldiers were to march over a given distance, the army moving the more rapidly would afford fewer soldiers per acre. The same is true of the ions. This will be true whether the ions move directly across from pole to pole, or only succeed in going across after many interruptions produced by their dissociating and recombining. The excess of negative ions is indicated in Fig. 2. If then there are more negative than positive ions between the carbon terminals, they would cause the potential of the gas to be lower than it otherwise would be, as is indicated in Fig. 1.

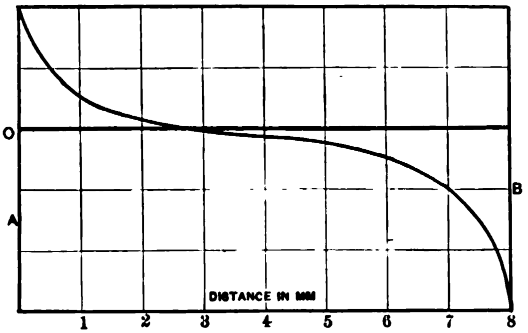


Fig. 2.

At the surface of contact between the gases and the carbons the conditions are somewhat exceptional. At *a*, Fig. 1, there would be no negative ions moving toward the right. If the potential gradient were the same here as elsewhere, there would be positive ions at *c*, which had come from the right, but no negative ions from the left to neutralize the electrostatic effect. The potential at *c* would, therefore, become positive, due to the positive ions, and the rise of potential between *a* and *c* would be greatly augmented. A similar effect would occur at the positive pole. This would account for the large fall of potential at the poles. The fact that the potential of the gas is lowered, because of the greater velocity of the positive ions would make the fall of potential at the positive pole greater than at the negative.

It is impossible to put this explanation into mathematical form, because of the complexity of the conditions. The amount of dissociation at any point depends on the temperature at that point according to some law which is not at present known, and the temperature in turn depends on the potential gradient which in turn depends on the amount of dissociation. It would be wiser to leave this circle of dependencies until our knowledge of the subject is in a more advanced state.

With this explanation of the arc it is easy to see why the apparent resistance becomes less as the current is increased. With increase of current there would be more heat developed in the arc. A greater dissociation would occur, and this in turn would cause the apparent resistance to be less. It would, however, be impossible for the potential difference to become smaller so fast that the amount of heat developed by a large current would be less than that developed by a smaller one. As far as is known this does not occur.

This view also harmonizes with the fact that with an alternating current the average potential between the poles is not zero, but is negative. The positive ions moving with greater velocity pass out of the field quicker, leaving an excess of negative ions and a corresponding negative potential.

The experiment referred to at the beginning of this article was suggested by a recent article in the *Philosophical Magazine*[1] by Professor A. P. Chattock on the velocity of ions in the case of discharge from a point to a plate. From his experiment there appears to be a drag produced on the air by the friction between the air and the ions, and this drag shows itself as an increased air pressure at the plate. Professor Chattock was able to measure this pressure by a sensitive manometer. It seemed possible to the present writer that if the current of the arc is carried by ions their presence could be detected by a similar method.

---

[1] Phil. Mag. (5), Vol. 48, p. 401.

The manometer used in this experiment was similar to one shown me at one time by Mr. W. R. Turnbull. I am unable to state whether any description is given of it elsewhere or not. It consists of an air-tight chamber, 4 cm. on each side and 1 cm. thick. One side is covered with rubber dam which moves with a very slight increase of pressure on the air within the chamber. The chamber is shown in cross section in Fig. 3. *A* is the chamber which may be connected at *e* to the other parts of the apparatus. *R* is the rubber dam covering one face. In front of the rubber is the mirror *m*. The mirror is fastened to the needle *n* which is free to rotate. *a* and *b* are strips of tin projecting from the chamber *A* and holding the needle *n* in place. The wire *d* is fastened to the rubber dam and also to the mirror near to the axis about which the mirror rotates, so that a slight motion of *d* gives the mirror a sensible rotation. This rotation was read by a telescope and scale. It has not as yet been possible to do any quantitative work, and accordingly the manometer was only roughly calibrated by comparing it with a water manometer. A pressure of 1 mm. of water corresponded to a deflection of 10 cm. with the sensitive manometer. A deflection of 1 cm. would therefore correspond approximately to a pressure of 1/100,000 of an atmosphere.

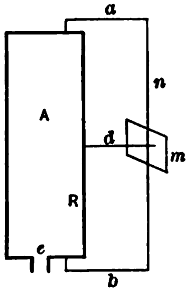


Fig. 3.

A small hole was drilled lengthwise through one of the carbons and the manometer was connected with this aperture.

On starting the arc the manometer showed an increase of pressure. It was, of course, impossible to keep the end of the arc on the same point on the carbon, and as a result there was a continual variation in the pressure indicated. The pressure was greater as the end of the arc was nearer the aperture. This continual variation of the pressure made any qualitative measurement impossible, but it was plain that the pressure was greater at the positive carbon.

In order to remove any doubt a manometer was constructed so as to measure the *difference* between the pressures at the positive and the negative carbons.

The instrument was the same as that shown in Fig. 3, except that in front of *R* was placed a second air-tight chamber. A window in this chamber allowed the motion of the mirror to be detected as before. One chamber was connected with the aperture through one carbon, and the other chamber to the aperture of the other carbon.

With this instrument the deflection was to the right or to the left depending on the direction in which the current passed between the carbons. The deflection was always reversed when the current in the arc was reversed. By this it was proved that the pressure at the positive carbon was greater than that at the negative. The deflection of the manometer was at times as much as 5 cm., showing that the difference of pressure was .00005 of an atmosphere.

The effect was very much more marked with a large current than with a small one. Thus when the current was 6 ampères there was scarcely any pressure indicated. When the current was 20 ampères an average reading of the manometer would indicate a difference in pressure of .00002 of an atmosphere. This was possibly due to the fact that the arc in the latter case covered a larger part of the carbon about the aperture.

The carbons were placed horizontal in order to avoid convection currents. But when they were placed other than horizontal no difference in the effect was noticed. Moreover, it seems impossible that the effect could be due to convection currents, because reversing the current without in any way changing the position of the carbons or their connections to the manometer would reverse the effect.

The effect may be due to the expansion of the gases in the arc caused by the heat, perhaps to the formation of CO_2 or to vaporization of the carbon. We should in this case expect the pressure at the positive carbon to be the greater, because the arc there is the hotter. This is, indeed, what we find. There is one objection to this view. If this effect were due to the heat of the arc, one would expect that when one carbon was directly over the other the upper carbon would show the greater pressure whichever way the current flowed. This was not found to be the case. The positive carbon showed the greater pressure whether it was up or down. Moreover, the experiment was tried of inclosing the carbons in a chamber from which free access of air was excluded as far as possible without stopping all vent for the expansion of the gases caused by the arc. The effect was in this case the same as when free access of oxygen was allowed. These cannot be considered as proofs that the effect is not due to differences of temperature, but they are at least facts which will warrant us in considering some other hypothesis.

Professor Chattock showed that in the case of discharge from a point to a plate $p = \frac{Cz}{V}$ where p is the total drag exerted on the air, C equals the total current, z equals the distance between the point and the plate and V equals the velocity of the ions for unit potential gradient. In the case of the arc the conditions are much more complicated. Instead of the current being all carried by ions of one sign, it is carried by both positive and negative ions. This in itself would not make the problem impossible of solution, if at all points the same part of the current were carried by ions of one sign. This, however, is not the case, and because of irregularities of the arc it is impossible to make any mathematical statement which will apply. But we can get some insight into the matter by considering the physical meaning of the equation $p = \frac{Cz}{V}$. The drag which the ions produce must be equal to the force exerted upon them since their acceleration is negligible. It is as it would be with a block being pulled over the floor with a uniform velocity. The pull on the floor due to the friction is the same as the pull on the block. So the drag on the air is equal to the total attraction between the plate and the ions which are at any instant above the plate. This will evidently be greater the greater the number of ions. Other things being equal the number of ions will be greater the slower they move. This is shown in the equation $p = \frac{Cz}{V}$. The number will be greater the more current is being carried and will be greater the greater the distance between the point and the plate. The drag will, therefore, be proportional to C and to z.

With this conception of the cause of the drag, *i. e.*, that it is caused by the attraction between the electrodes and the ions, we can apply it qualitatively to the arc. Referring to Fig. 2 we see that there must be a pressure on the positive carbon due to the attraction between the carbon and the negative ions near it. There will also be a pressure on the negative carbon for a similar reason. The attraction on the positive carbon will, however, be the greater because there are more negative ions between the carbons than positive ones. This, indeed, was what was found in the experiment.

It is thus seen that all the phenomena of the arc which are known can be explained in accordance with the hypothesis here suggested. The action of the arc is, however, very complicated, and it may well be that, though this explanation be partially correct, it is not a complete explanation. It is hoped that a more thorough investigation of the phenomena here noticed may bring to light facts more decisive, and result in definite knowledge concerning what has heretofore proved a troublesome problem.

COLGATE UNIVERSITY, January, 1900.

THE EFFECT OF MAGNETIZATION ON THE ELASTICITY OF RODS. II.

By James S. Stevens.

In the Physical Review for August, 1899, there was described a series of experiments in which by the use of interference methods, it was shown that iron and steel rods developed a greater elastic modulus when magnetized. In those experiments flexional elasticity alone was considered. In the present paper I shall describe some experiments which had for their object the determination of the effect of magnetization upon torsional elasticity. That some relation exists between torsion and elasticity has been known since 1847, when Matteucci examined the change in magnetization undergone by an iron rod when it was twisted. Further investigations have been carried on by Wertheim, Wiedemann, Kelvin and others, with the general conclusion that when a rod of soft iron

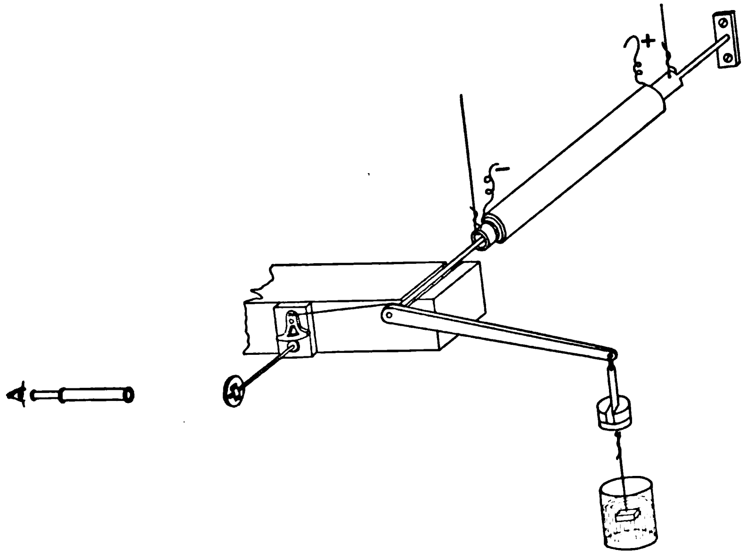


Fig. 1.

exposed to longitudinal magnetizing force was twisted, its magnetism was reduced by torsion in either direction (Ewing).

In these experiments three styles of cylindrical rods were employed: one of iron, 183 cm. long and 0.48 cm. in diameter; one of steel of the same dimensions, and lastly the iron rod was reduced in length to 163 cm. To magnetize these rods a coil of two layers of No. 16 wire was wound in 1,612 turns. The length of the coil was 132.1 cm. One end of the rod was rigidly screwed into an ron plate, fastened to a solid wall, and to the other was attached an arm carrying a weight at the opposite end which supplied the force of torsion. The arrangement of the apparatus is shown in the accompanying figure.

After the rod had been twisted by the application of the given weight the current was sent through the coil, and in every case where the current was sufficiently strong a *rising* of the weights was observed, showing that the magnetizing effect was such as to increase the torsional elasticity. In order to damp the vibrations a small weight suspended in a jar of water served as a dash pot. The following table shows approximately the angles through which the rods were twisted by the loads applied.

Moment of Torsion (dynes).	Angle of Torsion (degrees).
6.3×10^6	2
4.8×10^7	15
9.8×10^7	30
13.6×10^7	39

To measure the change which took place in the angle of torsion when the rods were magnetized, I employed two methods: first, a mirror was fastened to the arm and the rotation of the rod was measured by a telescope and scale; secondly, in case the rotation was small, a device shown in the figure was employed which multiplied the scale readings 9.43 times.

In the following tables are shown the current, magnetizing force, scale readings and the angle representing the corresponding rotation of the rods.

TABLE I.

Iron rod, 183 *cm. long. Moment of Torsion* 9.8 × 10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
7.7	0.01	0.3	50.0	0.94	25.5
13.7	0.02	0.6	54.6	0.97	26.3
19.9	0.12	3.3	71.9	1.1	33.0
23.0	0.29	7.8	76.5	1.2	36.0
29.1	0.45	12.3	79.6	1.2	36.0
32.1	0.66	18.0	91.8	1.2	36.0
35.2	0.72	19.2	104.0	1.3	39.0
38.3	0.74	20.1	117.8	1.3	39.0
42.8	0.83	22.5	127.0	1.3	39.0
44.3	0.87	23.7	137.7	1.4	42.0

TABLE II.

Iron Rod, 183 *cm. long. Moment of Torsion* 4.8 × 10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
4.6	0.01	0.3	36.7	0.90	24.6
9.2	0.02	0.6	39.8	0.92	25.2
13.8	0.23	6.6	47.4	1.00	27.3
16.8	0.35	9.6	52.1	1.06	28.8
19.9	0.46	12.6	64.3	1.13	30.9
23.0	0.62	16.8	76.5	1.22	33.3
26.0	0.69	18.9	82.3	1.23	33.6
30.6	0.78	21.3	90.3	1.25	34.2
32.1	0.80	21.9			

TABLE III.

Steel Rod, 183 *cm. long. Moment of Torsion* 9.8 × 10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
7.7	0.1	2.6	76.5	0.2	5.2
15.3	0.1	2.6	91.8	0.3	7.8
22.9	0.1	2.6	137.7	0.3	7.8
30.6	0.1	2.6	145.4	0.3	7.8
35.2	0.1	2.6	153.0	0.4	10.4
38.3	0.2	5.2	168.3	0.4	10.4
58.1	0.2	5.2	214.1	0.5	13.0
61.2	0.2	5.2			

Table IV.

Iron Rod, 163 *cm. long*. *Moment of Torsion* 9.8 × 10^7 *dynes*.

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
12.4	0.05	1.3	79.4	0.4	10.0
19.9	0.15	3.8	107.1	0.4	10.0
27.5	0.2	5.0	130.0	0.5	12.5
30.6	0.2	5.0	137.7	0.5	12.5
38.3	0.3	5.0	153.0	0.5	12.5
61.2	0.3	7.7	183.6	0.6	15.1
68.8	0.4	10.0	198.9	0.6	15.1
76.5	0.4	10.0			

Table V.

Iron Rod, 183 *cm. long*. *Moment of Torsion* 13.6 × 10^7 *dynes*.

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
33.7	0.01	0.6	91.8	0.67	18.3
47.4	0.04	1.2	183.6	1.14	28.2
53.6	0.08	1.8	229.5	1.14	31.2
56.6	0.26	7.2	306.0	1.17	31.8
64.3	0.48	13.2	382.3	1.19	32.4
82.6	0.58	15.9	405.5	1.22	33.3

Measurements were also taken with no weight added to the arm of torsion except the small one used with the dash pot. The moment of torsion was 6.3 × 10^6 dynes. While a deflection was noted with each magnetizing force used, the scale readings were quite irregular and the zero not constant.

By use of the device for multiplying the scale readings, it is thought that the error in estimation need not exceed eight seconds.

The above curves were plotted with magnetizing force divided by 4 and corresponding angles of rotation expressed in minutes as coordinates. *I* is for the steel rod, *II* the short iron rod, *III*, *IV* and *V*, the longer iron rod with moments of force 13.6 × 10^7, 4.8 × 10^7 and 9.8 × 10^7 dynes respectively. Without doubt, the lack of regularity of the lines is due in large measure to errors of observation.

From the general consideration of the experiments, and the inspection of the tables and plots, the following conclusions may, I think, be written down:

1. Magnetization of an iron or steel rod increases its torsional elasticity. This would be expected from the results of the experiments mentioned in the first part of this paper dealing with the effects of torsion upon elasticity.

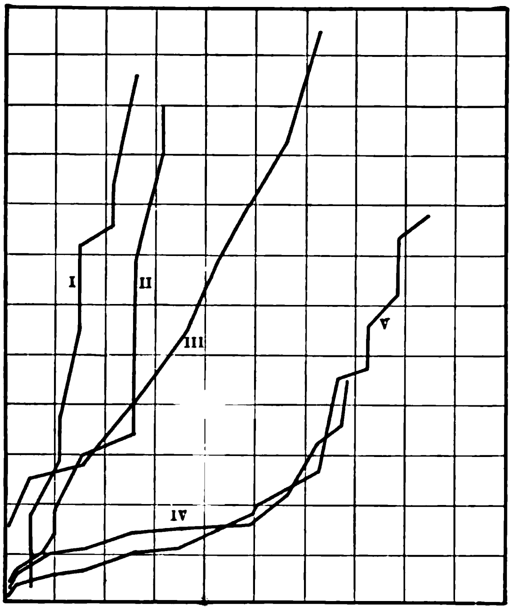


Fig. 2.

2. The effect is greater in iron than in steel rods of the same dimensions.

3. The increase in elasticity varies with the length of the rod.

If we compare the results of this experiment with those of the experiments on elasticity of flexion, we note a distinct agreement. In the first place the modulus of elasticity is increased in each case when the rod is magnetized; the increments are fairly proportional to the magnetizing forces; and they are inversely proportional to the magnitudes of original stresses. So far as elasticity of traction is concerned, the only results published are given in the Physical Review, Volume II., No. 4. They do not represent exact measurements but are inferences which were suggested in connection with another experiment, and go to show that in this case also magnetization increases the elasticity. It is expected that a little later the effect of magnetization upon a rod subjected to longitudinal pressure will be studied.

It ought to be said that the mechanical arrangements for this experiment are due to H. G. Dorsey, and that in taking the readings I was assisted by S. Sidensparker, instructors in the Department of Physics.

Physical Laboratory, The University of Maine.

THE INFLUENCE OF TEMPERATURE, PRESSURE, USED SOLUTIONS, AND SIZE OF ANODES ON THE DEPOSIT OF SILVER VOLTAMETERS.

By J. F. Merrill.

THE object of the work herein described was to see if by a change of temperature or pressure, etc., the electric charge carried by silver ions in the electrolysis of silver nitrate could be varied. It is well known that the amount of the silver deposit, as obtained in ordinary electrolysis, varies slightly according to the conditions under which the deposit is obtained. But that this variation is due to any change in the carrying capacity of the silver ions has probably not been proved.

The method here employed was of course comparative. Two silver voltameters, usually alike in all respects, were placed in series and the deposit from one under ordinary conditions compared with the deposit from the other obtained under different conditions. Then any differences in the two deposits must be due to the different conditions. After formation, the two deposits were treated alike with respect to washing, heating, etc., to avoid any variation in amount due to different treatment.

Influence of Pressure.

Preliminary experiments having shown that the pressure must be considerable to produce any certain effect, a piece of apparatus was made by means of which the voltameter could be subjected to a gaseous pressure up to 120 atmospheres. The pressure cage was made by boring a hole 3¼ inches in diameter in a piece of steel shafting, threading a portion of it, and then screwing it down upon a suitably prepared cast-iron base up through the bottom of which the conducting wires passed and the compressing gas was admitted.

The Rayleigh form of silver voltameters was employed. The diameter of each platinum dish, or bowl, was 7 cm., and the dimensions of the silver anodes, when new, were 4 cms. diameter by 2 mm. thick.

The insulation of the voltameter in the cage seemed to be perfect, but, to make sure of no error due to leakage, the position of the two voltameters with respect to the battery was reversed in alternate experiments.

The pressure was secured by admitting compressed air from a cylinder into the cage.

The following results were obtained :

No. Experiment,	1	2	3
Pressure in atmospheres,	103	95	103
Deposit in air,	1.1371	1.23745	1.08205
" under pressure,	1.1370	1.23740	1.08190
Difference in grams,	0.0001	0.00005	0.00015

The time of deposit in each case was about 32 minutes. According to these results, no pressure effect seems to have been produced, as the differences, though all in the same direction, are probably within the limit of experimental error.

To see if the air absorbed under pressure exercised any influence, two deposits were made after the voltameter had stood, in each case, during several hours under a pressure of 90–100 atmospheres. The differences in the deposits as obtained in air at ordinary pressure and under the hood, were 0.00000 and 0.00010 grams, respectively.

But it has been observed[1] that gases dissolved in the silver electrolyte affect the amount of the deposit. The result here reported may, however, be reconciled with these observations if it is remembered that oxygen and carbon dioxide decrease and nitrogen increases the deposit. Using air N, therefore, neutralizes the effect of O and CO_2.

Effect of Temperature.

In testing for a temperature effect, experience suggested a form of voltameter much more convenient for the purpose than the Rayleigh form. The electrolyte was contained in a glass beaker on the bottom of which rested the silver anode, while the disk-form kathode was suspended horizontally near the top of the electrolyte, just as the anode ordinarily is. The kathode was made of platinum wire 0.6 mm. in diameter, wound about cross-arms like the bottom of a basket into a close, flat spiral with the suspension wire coming out from the center perpendicular to the plane of the spiral. (If the electrolyte is a water solution a disk of sheet platinum makes a good kathode.) The current was led down to the anode by a plantinum wire passing through a small glass tube. In this form of voltameter no filter paper wrapping for the anode is necessary, and, for small currents, it is a cheap and satisfactory form in practice.

---

[1] Schuster and Crossley, Proc. Roy. Soc., 40, p. 344, 1892, also Meyers, Wied. Ann., 55, p. 288, 1895.

Here again the two voltameters were alike and were placed in series. The silver nitrate solution in one was boiled and then cooled in the same beaker to the temperature of melting ice, and the deposit from this compared with that obtained from a solution maintained at about 90°C. The object of boiling one solution and then cooling was so that both solutions should have nearly the same degree of freedom from dissolved gases.

In these experiments the value of the current was 0.25–0.3 ampère, and the area of each kathode about 13 sq. cm. The weights of deposits from five consecutive tests follow:

From hot solution,	0.79835	0.8434	0.82655	0.86470	0.85900
From cold solution,	0.79820	0.8435	0.82655	0.86420	0.85900
Difference,	0.00015	—0.0001		0.00050	

The average difference in these deposits is again within the limits of experimental error. But this result is apparently not in agreement with that of four observations made by Rayleigh, who found a difference of about 1 part in 2,500 in favor of the deposit from a solution at 50° C. as compared with that obtained from a solution at 4° C.; but it is in agreement with the result of two observations made by Kahle.[1] But if the deposit from a hot solution, or from a solution previously boiled and then cooled, was compared with that obtained from a cold, unboiled solution, the differences were irregular, the former being greater than the latter in amounts varying from 0.00–0.08 per cent. This was probably due to the expulsion of oxygen and CO_2 from the hot solution.

---

[1] Zeitschr. für Instr., 8, p. 229, 1898.

It is possible that there might be a slight increase in deposit at higher temperatures which was not revealed, owing to some of the deposited silver having been dissolved by the hot solution. Kahle showed that hot water would slightly dissolve the deposited silver. And this was verified by the writer who found that the kathode with its coating of silver lost 0.2 mg. by being suspended during two hours in water maintained at 90° C. Now, it may be that at the instant of deposition the silver is more soluble than after it has been washed and dried. However, an increase of temperature probably does not of itself increase or diminish the deposit, as will more fully appear below.

Though the deposits from cold and hot solutions, respectively, did not differ in amount, they did differ in physical appearance. Those from cold solutions were always fine grained and snowy white, while those from hot solutions were more coarsely granular with a tendency towards gray in color.

In a further search for a temperature effect, the deposit from a boiled and then cooled water solution was compared with that obtained from fused silver nitrate. The results of the last seven experiments are here given:

Temperature of $AgNO_3$	**260°**	**255°**	**250°**	**245°**	**243°**	**242°**	**243°**
Deposit from solution,	0.7224	.7307	.90250	.79955	.8511	.88965	.9068
Deposit from fusion,	0.7230	.73075	.90285	.79955	.85115	.89080	.90665
Difference,	0.0006	.00005	.00035		.00005	.00115	.00015

Though these results show a difference in favor of the deposit from the fusion, yet this is probably accidental and due to some particles of foreign matter that were not washed off; for there was always more or less of a scum floating on the surface of the fused silver nitrate, some of which adhered to the kathode when removed and was difficult to dislodge completely.

In all the experiments of this paper, the water solutions employed were made by dissolving c. p. silver nitrate in distilled water in the ratio of 15 parts to 85. When fresh, these solutions were all neutral to sensitive litmus paper. After each experiment the solution was usually poured back into the bottle from which taken through a funnel and filter paper; subsequently it was taken directly from the bottle for use.

EFFECT OF "USED" SOLUTIONS.

Two different series of tests were made on the relative deposits from fresh and "used" solutions. The voltameters (Rayleigh form) consisted of two platinum dishes 7 cm. diameter, with round silver discs 4 cm. by 2 mm. The first "used" solution tested was made November 27th, its volume was 300 c.c., and from it about 52 grams of silver had been deposited when this series began, January 16th. Five tests were made and 69 grams of silver had been deposited from the solution when the last test began. The deposits in different tests varied in amount from 1.5 gr. to 2.26 gr. and the current from 0.77 amp. to 1.0 amp. The result of the five closely agreeing measurements gave an average deposit from the old solution greater than that from the fresh by 1 part in 11600.

More than two months later another series of tests on this point was made, the old solution in the meantime having been used a great many times, portions of it repeatedly boiled, and about 55 grams more of silver deposited from it. In this series the deposit from the old solution was compared with that from two different fresh solutions and was greater in amount by about 1 part in 1700. Moreover, this was a percentage increase as was proved by currents of different intensities and deposits of different amounts. But in every case in both these series of tests, the deposit from the fresh solution was more coarsely crystalline (varying from slightly to decidedly) and slightly less white in color than that from the old solution—just the opposite of what Kahle observed. Furthermore, a test at the conclusion of the work with litmus paper and with litmus solution failed to show, with certainty, any acidity in either the fresh or "used" solutions. This again is not in agreement with Kahle's observations. But Prof. Morse, of the Chemical Department of Johns Hopkins University, who was kind enough to make acidity tests for me, gave it as his opinion that the solutions were about neutral.

Possibly the boiling to which the old solution had been repeatedly subjected prevented it from becoming acid. This explanation is suggested by the fact that another solution, made some months later, became slightly acid on repeated use without boiling. This latter solution would turn sensitive litmus paper, but still it was not acid enough to prevent the permanent existence of a precipitate in 100 c.c. of the solution, due to the addition of $\frac{1}{10}$ c.c. of tenth normal sodium hydroxide. This solution, therefore, was decidedly less acid than Kahle's, but perhaps it had not been used so much, for only about 42 grams had been deposited from the whole volume amounting to 600 c.c. But the solution was five months old when the acidity tests were made. Age alone, however, did not render it acid for a solution equally old, but unused, was perfectly neutral.

The increase in deposit from "used" solutions must be due to some reduction of the valence of the silver in the electrolyte. Whether this reduction is due to the passage of a current, to light and age, to age alone, or to all combined is a question which it is proposed to submit to the test of experiment.

Size of Anodes.

If, within ordinary limits, the relative size of anodes exercises any measurable influence it is important to establish this fact. It has been observed that larger deposits are often obtained in a large platinum dish than in a smaller one in series with the larger one. Schuster and Crossley expressed the opinion that this is due to the larger anode usually accompanying the larger dish.

To test this matter, the writer employed anodes of various sizes. At first two platinum dishes, each 7 cm. in diameter, were used, and the standard anode was a silver disc 4½ cms. by 3 mm. Two sizes of comparison anodes were tried one 3 cm. by 3 mm., and the other 1.8 cms. by 3 mm. The relative position of the anodes with respect to the battery was kept the same, but the position of the two bowls was alternated in each succeeding experiment. Thus the standard anode was alternately in each bowl, or dish.

Seven consecutive experiments with currents from 0.5–0.6 amp. and deposits of about 1.2 grams each led to the belief that there was no measurable effect due to the relative size of the anodes under the conditions of these tests. An unexpected result, however, appeared. In only one out of the seven tests were the deposits equal in amount. In the other six the deposit in one dish was larger than that in the other. But one dish was new and had a bright inner surface, whereas the other dish was old, its inner surface dull and scoured. In the six experiments there was deposited in the new dish a total of 7.1956 grams of silver and in the old one 7.19705 grams, which is 1 part in 5000 greater than the other. Now this difference could not be due to any relative error in the weights used to compare the deposits, for, in order to avoid this and obviate the necessity of making tedious corrections, the two dishes, by filing their edges, had previously been made very nearly equal in weight. Thus the bowls and their deposits were weighed with the same masses, respectively. These results seem to show, as Kahle has already observed, that the amount of the deposit is slightly dependent upon the nature of the surface receiving the deposit.

To test further for an anode effect, two platinum bowls, or dishes, each 9 cms. in diameter, the inner surfaces of which had been scoured, were used at kathodes, and the dimensions of the standard anode were 6 cms. by 2 mm. The comparison anode in the beginning was 1.8 cms. by 3 mm., but these dimensions of the small disc grew rapidly less, so that, at the end of the fourth test, the disc had been almost consumed, it weighing less than 1 gram.

The data follow:

No. test.	1	2	3	4
Amps.,	0.5	0.52	1.0	1.0
From large anode,	1.2303	1.0582	1.1592	1.1538
From small anode,	1.2304	1.0582	1.1591	1.1536
Difference,	—0.0001		0.0001	0.0002

These results seem to show that the relative size of the anodes, except possibly in extreme cases, has no appreciable influence.

Another point, tested by several experiments, was whether or not the filter paper wrapped about the anode in the Rayleigh form of the voltameter has any influence on the deposit. For this purpose a voltameter of the form previously used in the temperature experiments was connected in series with one of the Rayleigh form. J. H. Munktell's Swedish filter paper was used to wrap the anode.

It was found that the deposits were either the same in amount or their difference was within 0.2 mg.

The silver nitrate and anodes used in this work were obtained as c. p. from Eimer and Amend, New York.

The work was mostly done in the Physical Laboratory of the Johns Hopkins University, and the writer is indebted to the Directors of that Laboratory for valuable advice and assistance.

UNIVERSITY OF UTAH, SALT LAKE CITY.

A LECTURE DEMONSTRATION IN WAVE MOTION.

By Will C. Baker.

THE following is a description of a set of simple lecture demonstrations of the main phenomena of wave motion that have proved of great assistance to the writer in the past few years. As no description of them has been found in text-books or magazines, they are communicated in order that they may be of use to others. The experiments cover the same ground as the beautiful work of Vincent[1] and Wood,[2] but are adapted to lecture demonstration. The dimensions given are of small importance, but will serve to give an idea of the scale of the experiments.

A mercury tray—72 × 55 × 7 centimeters—whose smooth bottom is painted white, is filled to a depth of about half a centimeter with water, slightly blackened with ink. The proportion of ink to water will, of course, vary with the ink used, but the bottom of the tray must be clearly visible through the layer of fluid at rest. If now a beaker or other vessel of eight to ten centimeters diameter be placed in the tray a circular wave will be set up that will spread in all directions. The motion of this wave can be clearly followed as, the thickness of the fluid at the crest of the wave being much greater than at the trough, the appearance is that of a dark band travelling over a lighter background. The removal of the beaker gives a second wave; so, if the beaker be kept moving with a period of one or two seconds, a train of waves of period one-half to one second is produced.

It has been found best in lecturing to use either single waves or trains of not more than two or three, to avoid the interference effects after reflection at the sides.

Let a circular wave be set up as described. It travels across the tray at from fifteen to twenty-five centimeters per second and its reflection and the growth of the reflected wave at the sides of the tray can be easily followed on account of its slow motion. If a block of wood, say 50 × 3 centimeters by any thickness, be substituted for the beaker, a sufficiently good representation of a plane wave is obtained. Attempts have been made to eliminate the circular portions set up by the ends, by confining the disturbance between parallel guides, but the gain is so slight as not to warrant the loss in simplicity. With waves of this nature reflection of plane waves may be easily shown.

---

[1] Phil. Mag., June, 1897; Feb., 1898; Sept., 1898; Oct., 1899.

[2] Phil. Mag., Aug., 1899.

A semicircular portion of barrel hoop placed at one end of the tray forms a reflector illustrating convex and concave mirrors. A plane wave sent into its concave side converges to a focus, or a circular wave set up in its focus changes after reflection to a plane wave in a manner beautiful to see. Circular waves after reflection proceed to the conjugate focus. These actions are best followed, especially by the class, when single waves are used.

Refraction phenomena are shown as follows: A piece of window glass, say 60 × 12 centimeters, is placed about one millimeter beneath the surface of the fluid and parallel thereto, care being taken that its surface is thoroughly wet. The waves will travel more slowly over this shoal part than they will over the deeper parts of their path. Circular waves in passing over this, decrease in curvature and have a shorter wave-length (for trains of waves) than over the deep part, and in passing off again increase the curvature and lengthen out, as described in the laws of refraction; the displacement of the apparent center being clearly seen. Plane waves, as described above, at oblique incidences (say 60°) are refracted in passing onto the shoal, in a very marked manner.

A triangular piece of glass, of say 20 or 21 centimeter sides, with stops from its vertices to the sides of the tray to prevent disturbance from other parts of the wave front, enables us to show displacement of the image as with a prism; the depth of the fluid over the glass regulating the retardation of the wave.

A circular or lenticular piece of glass, immersed at a suitable depth permits us to watch the changes in curvature of the wave front at the various surfaces and its final convergence to a focus as in a lens. As the best results are obtained with a comparatively small retardation a circular or nearly circular sheet of glass of say 20 centimeters diameter is used.

Huyghen's principle may also be illustrated by placing across the tray a stop having a gap of about one centimeter at its center. Any wave striking on one side of this is reflected except at the gap, which becomes the source of a circular wave proceeding into the smooth water beyond.

The diffraction or bending of waves around obstacles can be easily shown.

To obtain good results the tray bottom must be smooth, of a light color and must be parallel with the water surface. Single waves permit the class to follow each one through its changes, except, of course, where one is dealing with changes of wave-length when trains, must be used.

As waves, and not ripples, are used any lecture table is steady enough for the demonstration.

It may be necessary to draw attention to the fact that in all the foregoing we are dealing with waves passing over surfaces and not through media but the mechanism of the actions are similar, if not the same, in the two cases.

The claim is made that this method permits the class to obtain vivid ideas of the transformation of waves under the conditions dealt with, and that "retardation" and the various other terms applied to wave phenomena tend to take on clear meanings.

PHYSICAL LABORATORY, QUEEN'S UNIVERSITY, KINGSTON, ONT.,
January 15, 1900.

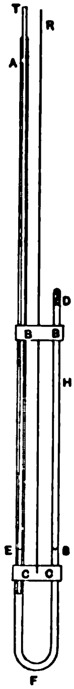
AN APPARATUS FOR THE DETERMINATION OF THE COEFFICIENT OF EXPANSION OF AIR AT CONSTANT PRESSURE.

BY C. F. ADAMS.

THE determination of the coefficient of expansion of air is a problem eminently suited to the physical laboratory of the secondary school, but no apparatus that has hitherto been recommended for it is capable of giving accurate results even in expert hands, much less in the hands of a high school pupil. The practice of allowing pupils to use apparatus and methods that at best can give only very rough approximations to the truth is prejudicial to the cause of science and injurious to the pupil; and yet a great deal of the so-called quantitative work in our schools is of such a character. Much of the apparatus that has been prominently recommended for this work is so crude that in the hands of pupils unaided the results obtained are almost meaningless, and such apparatus placed in the hands of the instructor often requires an expenditure of time and effort highly impracticable and after all without any adequate return for the outlay.

The apparatus here described, it is believed, is not open to these objections. It is the result of a long series of experiments by Mr. N. H. Williams, of the Detroit Central High School, with a variety of devices in most of which he attempted to use mercury as a valve to enclose the air, and in which he failed to secure accurate results because of trouble with moisture.

AFH is a glass tube about 80 cm. long and 3 or 4 millimeters internal diameter. It is closed at *D* by a piece of glass rod fused in. The bend of the tube contains sulphuric acid to a depth of about 10 centimeters. *BB* and *CC* are strips of thin brass bent around the two legs of the tube. *CC* can be slid up and down by the wire *R*, while *BB* serves merely as a guide and support for the wire. *T* is a glass tube whose external diameter is slightly less than the internal diameter of *AFH* so that it will slide easily into the tube *A* but fitting so closely that it will remain at any desired position. This tube *T* serves as a plunger in the acid by which the surface of the acid at *E* can be adjusted to the same level as the acid at *S*.



The apparatus is first placed in a deep cylinder of cold water, a thermometer being supported beside it. By means of the wire *R* and the tube *T* the upper edge of *CC* and the acid at *E* are adjusted to the level of the acid at *S*. It is then taken out of the water and the distance from the edge of *CC* to *D* is measured by the mirror scale of a Jolly balance. It is then placed in warm water and the adjustments and measurement repeated. The apparatus has been used with temperatures between 10° and 50° C. Care must be taken of course to stir the water so that the temperature at top and bottom of the cylinder shall be the same. The coefficient of expansion is obtained by the formula

$$a = \frac{v_2 - v_1}{v_1 t_2 - v_2 t_1}.$$

Sulphuric acid is used instead of mercury because with the latter it was found impossible to avoid trouble with moisture. The vapor tension of the acid at the temperatures at which the apparatus has been used is so small that it can be disregarded. The apparatus is extremely simple and inexpensive and such that it can be manipulated successfully by high school pupils. The following are the results obtained by one of our laboratory sections: 0.00342, 0.00360, 0.00368, 0.00357, 0.00368, 0.00370, 0.00376. Another section reported the following results at the close of the laboratory session: 0.00363, 0.00364, 0.00369, 0.00368, 0.00364, 0.00371, 0.00351, 0.00366. Indeed the accuracy of the results is such that it would seem that in the hands of more advanced students the apparatus would give results in which the expansion of the glass could be taken into consideration.

DETROIT CENTRAL HIGH SCHOOL, December 19, 1899.

ON THE CHANGE IN LENGTH OF SOFT IRON IN AN ALTERNATING MAGNETIC FIELD.

By Louis W. Austin.

WHILE doing some work with one of my students on the change of length of iron due to magnetism, it occurred to me that there must be a similar effect in an alternating field, but perhaps modified in amount or character by the more or less rapid alternations. If the change in length requires an appreciable time for its completion, such modifications, it would seem must exist, and the probability of this seemed to me great enough to make it worth while to investigate the subject experimentally.

The instrument used in the investigation is based on an old principle long used in the lecture room to show qualitatively the expansion of solids due to heat. In its original form it consisted of a small roller placed under one end of a bar, which was held fast at the other end. When the bar was heated it expanded the roller rolled and by the movement of a pointer or small mirror attached to it showed to the class the expansion of the bar. In the improved form the horizontal roller is replaced by a vertical hanging fiber or fine wire which is held between vertical plates of plate glass about 2 cm. square, so that it is rolled when one of the plates is moved just as one might roll a thread between the thumb and finger. Below the plates a larger glass fiber of sufficient rigidity is attached to the rolling fiber and this bears a mirror and water damper. A second rolling fiber is hung between the plates at a little distance from the first in order to keep the surfaces apart to prevent friction. In the first form of instrument made and the one used in this work the fixed plate was cemented to a block of wood and the movable plate against which the iron rested was pressed against it by means of a rubber band. With this arrangement I have been able to get results with a glass fiber 0.01 mm. in diameter, but in general fibers as small as this are extremely difficult to adjust and are very apt to break between glass surfaces. I am now experimenting with other surfaces in the hope of getting better friction contact and less trouble in adjustment. I hope soon to be able to communicate a paper on this work.

Most of the observations recorded in the present paper were taken with a glass fiber about 0.06 mm. in diameter, though for a few of the later ones a fine wire was used. The latter observations being reduced to the scale of the former on the supposition that the maximum elongation with direct current was the same in both cases. Each of the observations given in the tables is the mean of from six to twelve observed throws and the good behavior of the instrument may be judged from the fact that in none of the series used, except in the case of frequencies greater than sixty per sec., did any of the single observations differ by more than two per cent. from the mean. The absolute calibration was made rather roughly by noting the distance the cross hair of the telescope appeared to move on a scale half a meter away when the moving plate was pushed forward by a fine micrometer screw.

The preliminary observations on the change of length in an alternating field were made on a soft iron bar 26 cm. long and 0.7 cm. in diameter, the same one with which I had been working with a direct field. The source of current was the city incandescent lighting circuit having a frequency of 120 per sec. by day and 60 per sec. by night. I found that the curves showing the relation between change of length and strength of field were similar in form for the alternating and direct currents, but that the maximum in the case of the alternating field fell some ten or fifteen per cent. below that produced by direct field, and that this decrease was greater with the frequency 120 than with 60 per sec.

These experiments were rendered difficult by the large heat expansion produced by the Foucault currents and hysteresis which of course made accurate observation somewhat uncertain. I have thus far found no remedy for the latter difficulty, but have rid myself of the former for the most part by replacing the solid iron bar by a bundle of wires of soft Swedish iron firmly bound together and insulated with varnish. The observations given in this paper were taken with such a bundle consisting of ten wires 27.5 cm. long and 1.4 mm. in diameter. The magnetizing solenoid was 24 cm. long with 12 turns per cm. The dilatometer with the fiber used (about 0.06 mm.) gave a magnification of about 127000 times with the scale 360 cm. distant. This gave a maximum reading of 14.5 cm. on the scale with direct field.

With apparatus kindly placed at my disposal by the department of electrical engineering[1] I was able to work with currents varying in frequency from 10 to 131 per sec. The values of H varied from 7.5 to 375, approximately, no correction being made for the influence of the ends of the solenoid. The larger value was a little more than enough to bring the iron back to its original length. With the stronger fields, especially with the higher frequencies, the heat due to hysteresis rendered the observations somewhat uncertain. I have been able, however, within the limits of field and frequency used, to get very consistent results by taking the readings by breaking the circuit and noting the distance the cross hair appeared to spring back on the scale. This motion is so sudden and so dead-beat that I do not think any larger error is introduced by the heat drift. Thinking that the water damper might influence readings taken in this way, several observations were taken without it, but no change in the throws could be detected.

In Table. I are given the changes in length expressed in ten-mil-

TABLE I.

H	Direct Current.	10 per sec.	30 per sec.	60 per sec.	120 per sec.
7.5	1.7	2.2	2.2		
15.0	9.0	13.2	13.2	14.4	13.2
30.0	29.5	32.4	29.0	25.8	24.5
45.0	38.5	34.5	35.0	31.0	29.4
75.0	41.5	39.8	36.6	34.1	31.0
150.0	30.1	28.0		27.0	
225.0	20.2	18.3			
300.0		8.0			
375.0	−2.8	−4.0?			

---

[1] My special thanks are due to Mr. M. C. Beebe, instructor in electrical engineering, for his kindness in managing the electrical machinery used.

lionths of the length for direct current and for currents with alternations of 10, 30, 60 and 120 per sec., the strength of field varying from 7.5 to 375 C. G. S. The maximum comes at a value of H of about 75 in all the series, and the general course of the phenomenon is similar in all. With the lower values of H the elongations seem to increase more rapidly with the alternating currents than with the direct. This is perhaps due to the fact that the currents were read with an ammeter which indicated the square root of the mean square of current strength, which is very probably less than the effective strength of the alternating currents in this phenomenon. With all values of H above 15 the alternating current elongations fall below those with direct current. The curves of Fig. 1 show these relations graphically for direct current and frequencies of 60 and 120.

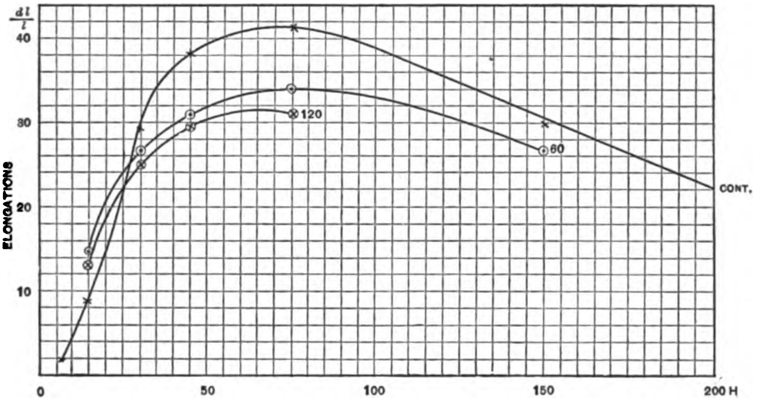


Fig. 1.

TABLE II.

Frequencies.	Maxim. expansion.	Frequencies.	Maxim. expansion.
Direct current	41.5	66 per sec.	33.2
10 per sec.	39.8	81	32.2
16	39.6	99	31.5
30	36.6	120	31.0
40	35.2	131	30.7
60	34.0		

Table II. gives the maximum elongation corresponding to different frequencies. A steady drop is noted as the frequencies become higher, which becomes less marked toward the end. Fig. 2, with its rather flat approximately exponential curve, shows the character of the change much better than the table can do.

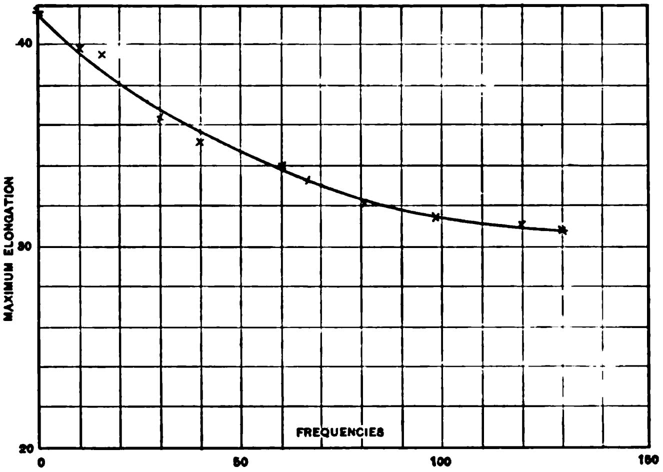


Fig. 2.

An attempt was made at one time to use the secondary currents of a transformer having a Wehnelt interrupter in the primary. The frequency as estimated from the pitch was about 800 per sec. The heating effects were so great, however, that no readings of value could be taken, the scale going by the eye far too rapidly to be read. The maximum certainly fell much below any of the values given in the table and was estimated to be about half that found with the frequency of 131. The results of the investigation may be briefly stated as follows:

1. The general course of the change in length of soft iron in an alternating magnetic field is the same as in the direct field.

2. For all values of H above 25, and at least as high as 375, the length of the soft-iron specimen is less with an alternating than with a direct field.

3. The maximum elongation gradually becomes less as the frequency of the alternations is increased.

This continued decrease of the maximum elongation with increasing frequency is perhaps the most interesting fact brought out. It appears as though there were an appreciable viscosity[1] in the molecular movements depending on the rapidity of the alterations. The last two results may, it seems to me, have a connection with the observations of several physicists[2] on the decrease of permeability of iron when subjected to rapid magnetic reversals. The recent work of Niethammer[3] and of Wien[4] on this subject also shows a correspondence with the third result, Niethammer finding a percentage decrease in permeability with increasing frequency, agreeing fairly closely numerically with the results in this paper. Wien's percentage of decrease is considerably smaller. It can hardly be said, however, that the decrease in the maximum expansion is due merely to decreased induction, since if this were so it would seem that an increase of the field should raise the induction so as to produce the same maximum elongation as before, the only result being to make the maximum correspond to a slightly greater value of *H*. Since this is not the case the decreased maximum elongation cannot be considered a direct result of the decreased permeability, but their quantitatively similar decrease would indicate that they both may follow from the same molecular causes.

Exactly what course the elongation curves take with higher values of *H*, I have not been able to discover on account of the heating disturbances. The observations merely indicate that with the alternating current the iron is brought back to its initial length at a lower value of H than with the continuous current. Of the course of the phenomenon beyond this point I can say nothing, but I hope in the near future to overcome the difficulties which have restricted the scope of the work and to investigate the effects both of stronger fields and higher frequencies.

---

[1] Since the completion of this work I have come across the observations of A. M. Mayer (Phil. Mag., 1873, Vol. 46, p. 177) on the time required for the full magnetic expansion to take place. These are the only observations made up to this time, so far as I am aware, that bear on the time factor in this phenomenon. He found that with his strongest current (25 Bunsen cells) $\frac{1}{10}$ second was required for the expansion and $\frac{3}{10}$ second for the corresponding retraction. While these estimates are certainly rough, they indicate the presence of the same appreciable viscosity which shows itself in the alternating field.

[2] Warburg and Hönig, Wied. Ann., Vol. 20, 1883, p. 814.

Tanakadate, Phil. Mag., Vol. 28, 1889, p. 207.

Klemenčič, Sitzungsber. d. k. Gesellsch. d. Wissench. zu Wien, Vol. 103, p. 17, 1894.

Weihe, Wied. Ann., Vol. 61, 1897, p. 578.

[3] Niethammer, Wied. Ann., Vol. 66, 1898, p. 29.

[4] M. Wien, Wied. Ann., Vol. 66, 1898, p. 859.

PHYSICAL LABORATORY OF THE UNIVERSITY OF WISCONSIN, January 10, 1900.

NEW BOOKS.

A Text-Book of Physics. By G. A. WENTWORTH and G. A. HILL. Pp. 440+vii. Ginn & Co., Boston. 1898.

The book before us is a disappointment. The authors have won an enviable reputation as writers of mathematical text-books, but in this book they show most convincingly that they are treading upon unfamiliar territory. True, there are many points about the book that deserve praise, and in the hands of a teacher who is a trained physicist the book may be useful; but to those who are only slightly acquainted with the subject, and these are the ones who will use the book most, it will be an unsafe guide. Errors for which it is difficult to find any excuse are unpleasantly frequent. A partial list of these has been prepared by the publishers, and is now sent with the book. Since this review must be brief a complete analysis of the book cannot be given, and the reviewer must content himself with giving a few of the reasons which have led him to the conviction that the book is an undesirable addition to our list of text-books.

On p. 19 we find the venerable fable that "Whatever the nature of the body or the kind of strain, the following simple law holds true: Up to the elastic limit strain varies directly as stress." That this may be branded on the memory of the pupil the same statement is repeated further on and three illustrative problems are given. But the fact is, as most physicists know, that this law of Hooke has been shown to be only a rough approximation.

In the chapter on heat among other minor errors is the statement, twice repeated on p. 102, that "the coefficient of expansion of all gases is equal to .00366" when even elementary books on the subject show that the coefficients for different gases lie all along the way from the value named up to about .00390.

In the discussion of the steam engine we find this statement regarding Carnot's principle: "A denial of its truth would lead to the absurd conclusion that energy can be created out of nothing." It leads to nothing of the sort. The law of the conservation of energy is no more affected by Carnot's principle than is the law of Moses, as was shown by Maxwell a generation ago. The authors state that our best engines "never have a greater efficiency than $\frac{1}{10}$." Ewing begs to differ.

In the treatment of electricity and magnetism the authors follow the beaten track of a generation ago, and the student finishes this portion of the subject without knowing that the medium surrounding a magnet or conductor plays any part. Such an important term as *strength of pole* is erroneously defined on p. 264 and a definition on p. 307 indicates that the international ohm has not yet been heard of. To Faraday alone is given the honor of having discovered induced currents—Henry is not mentioned. On p. 346 energy and power are confounded in the statement "Electric energy is the product of two factors, current strength and electromotive force." The shade of Ruhmkorff would be interested to learn that he and not Fizeau had "improved the induction coil by adding a condenser," but his feeling of pleasant surprise would give place to righteous indignation on being accused further on of having used in it "paraffined mica."

In judging any book one must know its aim. The aim of this book as stated in the preface is "to give a rational explanation of the more important physical phenomena, and to prepare the way for further investigation and study of physical science." If this was the aim at the start it seems to have been forgotten when the last division of the subject, light, was taken up. Reflection and refraction are treated without a word of reference to the wave theory; that is to say, these phenomena are *described*, not explained. Not only are explanations omitted but in many a place the facts are incorrectly stated. Sometimes the errors are typographical, at other times the authors are clearly at fault; for instance, in the article discussing the colors produced by thin films they say "According as this distance is equal to the wave-length or to half the wave length the rays will reinforce or destroy each other." Here they leave out of account the fact that there is at one point a change of phase of half a wave length, and of course, come to a conclusion exactly the reverse of the truth. Some of the optical diagrams are useless and that of the opera glass in particular is misleading and indefensible.

In determining how much space should be given to different subjects it is not to be expected that any two men will agree. Still an author who devotes no less than eight pages of his book to mirrors ought to have found space to mention the diffraction grating. A whole page is devoted to the bicycle while the important subject of thermal electricity has to be content with half a page. Half a page though seems to be more than the authors can write without drawing upon their imaginative faculties. They make the bismuth-antimony couple seven and a half times as powerful as it really is and further say that "for the same difference of temperature, bismuth and antimony give a stronger current than any other combination," a statement that needs some qualification in view of the fact that at least two other couples have much higher electromotive forces. In other cases that might be mentioned the authors make statements that are not sufficiently guarded.

One of the truly original ideas of the authors is expressed on p. 157, namely, that the diffusion of two gases illustrates adhesion; but in a text-book accuracy is preferable to originality of this sort. In conclusion the reviewer wishes to express his conviction that any man who writes such a text-book as this, so abounding in sins, both of commission and omission, is deserving of censure. To cast a stumbling block in the path of the young is no light offense.

JOSEPH O. THOMPSON.
AMHERST COLLEGE.

The Elements of Alternating Currents. By W. S. FRANKLIN and R. B. WILLIAMSON. 8vo, pp. 212. New York, The Macmillan Co., 1899.

This book is valuable, not only for what it contains but especially for what it omits, and its authors have succeeded admirably in presenting a clear outline of the essentials of alternating current theory and practice in a concise form, which may serve as a basis for a course of instruction in those subjects. It is so arranged that its treatment of the various divisions of the subject may be conveniently developed by the instructor in one direction or another as the circumstances under which the book is used may require.

Essentially a book for class instruction of electrical engineering students, it will, in an extended course, require amplification by the instructor, as obviously contemplated by the authors, for there has been no attempt to cover in the text many features of more or less importance. The fact that little else than the elements of the subject is included in the work serves to emphasize the fundamental conceptions in a particularly useful manner.

The book has many illustrative problems of a valuable character, together with their solutions, and at the close of most of the chapters are collections of problems for solution by the student which are well calculated to fix in the mind the principles of the chapters to which they are appended.

The first five chapters introduce the reader to the fundamental conceptions of monophase alternating current circuits and to the machines, instruments and measurements required for such circuits.

Chapters VI. and VII. present a satisfactory outline of the use of complex quantities and a number of illustrative problems which may serve as a basis for, and a valuable introduction to those methods which Mr. Steinmetz has so firmly incorporated in modern electrical engineering calculation.

Chapter VIII. introduces the conceptions of polyphase circuits and generators, while chapters IX., X. and XI. present in a more detailed manner the elements involved in the design of alternators and transformers.

The synchronous and induction motors and the rotary converter receive a satisfactory, though brief, treatment in chapters XII., XIII. and XIV., and in chapter XV. is given an exceedingly concise consideration of transmission lines.

The work as a whole is illustrated with well executed and pertinent diagrams, the typography is excellent, and the few errors in the types noted are to be expected in a first edition. The appearance of the book at the present time meets a distinct demand for a systematic outline on which a lecture or recitation course of instruction may proceed to any development required.

HAROLD B. SMITH.

De l'influence de la pression sur les actions chimiques. By GEORGES AIMÉ (1837), 12 pp.; with an introduction by P. DUHEM. 19 pp. Paris, A. Hermann, 1899.

The introduction is entitled *A point in the history of the Sciences. The tension of the dissociation before Sainte-Claire Deville.* Duhem has done a distinct service in bringing about the republication of this remarkable Thesis of Georges Aimé which, as he states, was brought to his notice by M. G. Brunel, Dean of the Faculty of Sciences at Bordeaux.

Duhem gives a biographical sketch of the author and a list of his publications. The remainder of the introduction is an interesting comparison of the work of Georges Aimé with the much later work of Sainte-Claire Deville and of Debray. The perusal, says Duhem, of the thesis of Aimé shows clearly that this physicist had reached the notion of the *tension of dissociation* in the form in which this notion grew out of the experiments of Debray thirty years later.

At the end of the eighteenth century Lavoisier had advanced the idea that pressure opposed the disaggregating force of heat so that at a given temperature a certain pressure would suffice to stop vaporization. Hutton in 1798 applied Lavoisier's idea to the explanation of the geological formation of marble by the melting of limestone without decomposition, the pressure due to superimposed strata being thought of as counteracting the tendency to decomposition. In 1804 the celebrated experiment of James Hall was communicated to the Royal Society of Edinburgh. This experiment was the melting of amorphous calcium carbonate under pressure and the formation of crystalline carbonate. In 1802 Dalton showed that the pressure which equilibrates the vaporizing tendency of a liquid is the pressure of its vapor only, a result which greatly extended the ideas which Lavoisier had previously advanced concerning the influence of pressure on vaporization.

What Dalton had done in extending the ideas of Lavoisier, Georges Aimé did in extending the theory of Hutton. He undertook to demonstrate that when a substance is decomposed by heat it is not the total pressure which is effective in arresting the decomposition, but only the pressure due to the particular gas which is generated by the decomposition.

Aimé showed in the first place that the decomposition of a carbonate by an acid in a closed vessel is arrested when the carbon dioxide formed reaches a certain pressure which depends upon the nature of the carbonate and of the acid. In the second place he showed that this pressure is independent of the proportions of carbonate and of acid. In the third place he showed that the arrest of the decomposition was determined by the pressure due to carbon dioxide only. He made no explicit statement concerning the dependence of this dissociation pressure upon temperature, although he must have considered the question, inasmuch as he was familiar with Hutton's theory and with Hall's experiment.

These results of Georges Aimé were entirely lost sight of, and it was in 1857 that Sainte-Claire Deville began again to approach the subjects Sainte-Claire Deville's experiments did not completely verify his hy. pothesis of a definite dissociation pressure, which hypothesis he advance- in 1863 when he had reached a clear notion of the matter, and it wad not until the researches of Debray were published in 1867 that this hypothesis became a matter of fact.

W. S. Franklin.

Traité élémentaire de Mechanique chemique fondée sur la Thermodynamique. By P. Duhem. Vol. IV., 381 pp. Paris, A. Hermann, 1899.

With this fourth volume Duhem's Treatise is completed in about fourteen hundred pages, constituting a most complete outline of the theory of the application of thermodynamics to chemistry. The first, second and third volumes of this Treatise have been previously noticed in the Physical Review. This treatise represents an immense amount of labor on the part of the author and an exceptional breadth of knowledge of the subject.

It is characteristic of the application of thermodynamics to chemistry that it formulates the subject out of all proportion to our real knowledge, introducing unknown functions and a meaningless plurality of generalized coördinates. This monumental work of Duhem's is, therefore, of less use to the experimentalist than an equally laborious and erudite treatise might be in a subject lending itself less readily to the unbridled play of mathematical generalizations.

In this fourth volume the author discusses what he calls *les Melanges doubles* (double mixtures), meaning systems of two phases of variable composition, such, for example, as alcohol and water liquid and vapor.

W. S. FRANKLIN.

American Telephone Practice. By KEMPSTEAD B. MILLER. New York, American Electrician Co., 1899. Pp. x + 458.

In this volume Mr. Miller offers a wealth of detailed information concerning the systems of telephone construction in the United States. There is but little about the theory of the telephone, and that of the most elementary character, but the description of all the varied parts that go to make up the telephone exchange as it exists at the present day will be found of the greatest interest to all who care for the modern industrial development of electricity. One who is not already familiar with the subject cannot but be struck at the bewildering multiplicity of detail involved in the practical working of a device which when reduced to its essential features is so remarkably free from complicated mechanism as the telephone.

The opening chapters of Mr. Miller's volume lead the technical reader over very familiar ground. The description of the various forms of telephone receivers and transmitters, of batteries and of calling apparatus, together with the properties of circuits, are topics which have been treated in nearly every work on applied electricity which has hitherto appeared. Chapters 14 to 25, however, which form the bulk of the present volume, are entirely devoted to the means by which those who desire to communicate by telephone are brought together. In these chapters simple switchboards for small exchanges, listening and ringing apparatus for switchboards, self-restoring switchboard drops, complete switchboards for small exchanges, lamp signal switchboard, the multiple switchboard, transfer systems, common battery systems, house systems, etc., are treated in a very complete manner with diagrams of the wiring, and excellent illustrations of the more important pieces of apparatus. These follow a series of chapters on the various types of party lines, and the book concludes with a treatise on the selection of wire for telephone use, on the construction of pole lines, and on the laying of underground cables, together with a chapter on line and cable testing.

E. L. N.

Standard Text-Books on General Physics

Elements of Theoretical Physics

By **DR. C. CHRISTIANSEN**
Professor of Physics, University of Copenhagen
TRANSLATED INTO ENGLISH BY
W. F. MAGIE, Ph.D., Professor of Physics, Princeton University
Cloth. 8vo. $3.25, net

Problems and Questions in Physics

CHARLES P. MATTHEWS, M.E., AND **JOHN SHEARER, B.S.**
Purdue University *Cornell University*
8vo. Cloth. pp. 247 + 4. Price $1.60, net

An Intermediate Course of Practical Physics

By **A. SCHUSTER, Ph.D., F.R.S.,** AND **C. H. LEES, D.Sc.**
Owens College, Manchester *Owens College, Manchester*
12mo. Cloth. pp. xv + 248. Price $1.10, net

Lessons on Elementary Practical Physics

By **BALFOUR STEWART, A.M., LL.D., F.R.S.,** AND **W. W. HALDANE GEE**

Vol. I. **General Physical Processes.** 12mo. $1.50, *net*
Vol. II. **Electricity and Magnetism.** $2.25, *net*
Vol. III. Part I. **Practical Acoustics.** $1.10, *net*
Part II. **Heat and Light.** *In Press*

Laboratory Manual of Physics and Applied Electricity

Arranged and Edited by **EDWARD L. NICHOLS**
Professor of Physics in Cornell University
IN TWO VOLUMES
Vol. I. Cloth. Price $3.00, net

JUNIOR COURSE IN GENERAL PHYSICS. By ERNEST MERRITT and FREDERICK J. ROGERS.
Vol. II. Cloth. pp. 444. Price $3.25, net

SENIOR COURSES AND OUTLINES OF ADVANCED WORK By GEORGE S. MOLER, FREDERICK BEDELL, HOMER S. HOTCHKISS, CHARLES P. MATTHEWS, and THE EDITOR.

A large proportion of the students for whom primarily this Manual is intended are preparing to become engineers, and special attention has been devoted to the needs of that class of readers. In Vol. II., specially, a considerable amount of work in applied electricity, in photometry, and in heat has been introduced.

THE MACMILLAN COMPANY
New York: 66 Fifth Avenue
BOSTON CHICAGO SAN FRANCISCO

STANDARD BOOKS ON ELECTRICITY AND MAGNETISM

The Theory of Electricity and Magnetism

By A. G. WEBSTER, A.B. (Harv.), Ph.D. (Berol.)

Assistant Professor of Physics, Director of the Physical Laboratory, Clark University, Worcester, Massachusetts

8vo. Cloth. pp. xii + 576. Price, $3.50, net

Elementary Lessons in Electricity and Magnetism

By SILVANUS P. THOMPSON, D.Sc., B.A., F.R.A.S.

New Edition. With Illustrations. 16mo. Cloth. $1.40, net.

"I consider Thompson the best book of its size and nature on the subject of electricity, and have had frequent occasion to recommend it to friends and students. It is the only book that is not simply a earrangement or reproduction of the old treatise."

—William A. Hallock, Ph.D., *Columbia University, New York City.*

The Principles of the Transformer

By FREDERICK BEDELL, Ph.D.

Assistant Professor of Physics in Cornell University

8vo. Cloth. 250 Illustrations. Price, $3.25, net

This work constitutes a systematic treatise on the alternating Current Transformer, and a logical exposition of the principles involved, of the most modern methods of transformer design, construction, and testing, and of transformer systems of distribution.

A Text-Book on Electro-Magnetism

AND THE CONSTRUCTION OF DYNAMOS

By DUGALD C. JACKSON, B.S., C.E.

Professor of Electrical Engineering, University of Wisconsin

12mo. Cloth. Price, $2.25, net

Alternating Currents and Alternating Current Machinery

By DUGALD C. JACKSON, C.E. and JOHN P. JACKSON, M.E.

University of Wisconsin — *Pennsylvania State College*

12mo. Cloth. pp. xvii + 729. Price, $3.50, net

"I like the way in which they develop their theory and make practical applications. I feel sure at the book will receive a hearty welcome"

—Geo. W. Patterson, Jr., *Asst. Professor of Physics, University of Michigan*

The Alternating Current Circuit

By W. P. MAYCOCK, M.T.E.E.

An Introductory and Non-Mathematical Book for Engineers and Students. With 51 Illustrations, Index, and Ruled Pages for Notes.

16mo. Cloth. pp. viii + 102. Price, $1.00

The Storage Battery

A Practical Treatise on the Construction, Theory and Use of Secondary Batteries,

By AUGUSTUS TREADWELL, Jr.

Cloth. 12mo. pp. xx + 257. Price, $1.75, net

JUST READY

Electricity in Town and Country Houses

By PERCY E. SCRUTTON.

Second Edition. Fully Illustrated. 12mo. Cloth. pp. xii + 148. Price, $1.00.

A brief and simple statement of the first principles of the application of electricity to the needs of the home, for heating, cooking, lightning, etc. A description is given of the plants required for towns of various size, and there are numerous illustrations showing examples of electric appliances. It is a popular and untechnical book, and intended for those who have not the time necessary for a study of more strictly scientific works.

THE MACMILLAN COMPANY

New York: 66 Fifth Avenue

BOSTON CHICAGO SAN FRANCISCO

THE CHEAPEST BOOKS IN

Science, Philosophy and Psychology

Now Publishing in America

High Grade Paper. Large Print. Stitched Backs

THE RELIGION OF SCIENCE LIBRARY

No. 1. **The Religion of Science.** By PAUL CARUS. 25 cents (1s. 6d.).
2. **Three Introductory Lectures on the Science of Thought.** By F. MAX MÜLLER. 25 cents (1s. 6d.).
3. **Three Lectures on the Science of Language.** By F. MAX MÜLLER. 25 cents (1s. 6d.).
4. **The Diseases of Personality.** By TH. RIBOT. 25 cents (1s. 6d.).
5. **The Psychology of Attention.** By TH. RIBOT. 25 cents (1s. 6d.).
6. **The Psychic Life of Micro-Organisms.** By A. BINET. 25 cts. (1s. 6d.).
7. **The Nature of the State.** By PAUL CARUS. 15 cents (9d.).
8. **On Double Consciousness.** By ALFRED BINET. 15 cents (9d.).
9. **Fundamental Problems.** By PAUL CARUS. Pp., 373. 50 cents (2s. 6d.).
10. **The Diseases of the Will.** By TH. RIBOT. 25 cents (1s. 6d.).
11. **The Origin of Language** and **The Logos Theory.** By LUDWIG NOIRÉ. 15 cents (9d.).
12. **The Free Trade Struggle in England.** By GEN. M. M. TRUMBULL. 25 cents (1s. 6d.).
13. **Wheelbarrow on the Labor Question.** 35 cents (2s.).
14. **The Gospel of Buddha.** By PAUL CARUS. 35 cents (2s.).
15. **Primer of Philosophy.** By PAUL CARUS. 25 cents (1s. 6d.).
16. **On Memory,** and **The Specific Energies of the Nervous System.** By PROF. EWALD HERING. 15 cents (9d.).
17. **The Redemption of the Brahman.** By R. GARBE. 25 cents (1s. 6d.).
18. **An Examination of Weismannism.** G. J. ROMANES. 35 cents (2s.).
19. **On Germinal Selection.** By AUGUST WEISMANN. 25 cents (1s. 6d.).
20. (Out of print.)
21. **Popular Scientific Lectures.** By ERNST MACH. 50 cents (2s. 6d.).
22. **Ancient India. Its Language and Religions.** By PROF. H. OLDENBERG. 25 cents (1s. 6d.).
23. **The Prophets of Israel.** By PROF. C. H. CORNILL. 25 cents (1s. 6d.).
24. **Homilies of Science.** By DR. PAUL CARUS. 35 cents (2s.).
25. **Thoughts on Religion.** By G. J. ROMANES. 50 cents.
26. **The Philosophy of Ancient India.** By R. GARBE. 25 cents (1s. 6d.).
27. **Martin Luther.** By GUSTAV FREYTAG. 25 cents (1s. 6d.).
28. **English Secularism.** GEORGE JACOB HOLYOAKE. 25 cents.
29. **On Orthogenesis.** By TH. EIMER. 25 cents (1s. 6d.).
30. **Chinese Philosophy.** By PAUL CARUS. 25 cents (1s. 6d.).
31. **The Lost Manuscript.** By GUSTAV FREYTAG. 60 cents (3s.).
32. **A Mechanico-Physiological Theory of Organic Evolution.** CARL VON NÄGELI. 15 cents (9d.).
33. **Chinese Fiction.** By DR. GEORGE T. CANDLIN. 15 cents (9d.).
34. **Mathematical Essays and Recreations.** By H. SCHUBERT. 25 cents (1s. 6d.).
35. **The Ethical Problem.** By PAUL CARUS. 50 cents (2s. 6d.).
36. **Buddhism and its Christian Critics.** PAUL CARUS. 50 cents (2s. 6d.).
37. **Psychology for Beginners.** HIRAM M. STANLEY. 20 cents (1s.).
38. **Discourse on Method.** RENE DESCARTES. 25 cents (1s. 6d.).
39. **The Dawn of a New Era.** PAUL CARUS. 15 cents (9d.).
40. **Kant and Spencer.** PAUL CARUS. 15 cents (9d.).

THE OPEN COURT PUBLISHING COMPANY
324 Dearborn Street, Chicago, Ill.
LONDON: Kegan Paul, Trench, Trübner & Co., Ltd.

QUEEN INSTRUMENTS

New
Queen Wall Pattern
Galvanometer

The **Moving Coil** in this **Galvanometer** is of **small diameter** and light weight. It can be made **dead beat** and will then give **readings** with much greater **rapidity** than other forms on the market. When arranged for **Ballistic** work the **period** of **swing** can be made as desired.

PHYSICAL APPARATUS INDUCTION COILS
ELECTRICAL TESTING INSTRUMENTS

QUEEN & CO., INC.
J. G. GRAY, Pres.

Electrical and Scientific Instruments

59 Fifth Avenue, New York
480 Monon Bldg., Chicago
1010 CHESTNUT ST., PHILADELPHIA

Volume X. *April, 1900.* *Number 4.*

THE

PHYSICAL REVIEW.

THE ACOUSTIC ANALYSIS OF THE VOWELS FROM THE PHONOGRAPHIC RECORD.

By Louis Bevier, Jr.

THE use of the phonograph in the scientific study of vowel timbre is not new. It began more than twenty years ago when the instrument was still in an undeveloped stage, and much less true to timbre than it is today. One difficulty has been to secure a sufficiently magnified trace of the record for convenient study. The apparatus of Jenkin and Ewing[1] magnified ordinates 400 times, but was a machine of some complexity, and too delicate for general use by investigators. Its records, except in the hands of laboratory experts, would be untrustworthy.

Since that time many attempts have been made with more or less success, but I know of no apparatus so simple and inexpensive as that by means of which I have obtained, with the aid of my colleague, Dr. F. C. Van Dyck, Professor of Physics in Rutgers College, vowel-curves of very great clearness, and in which the ordinates are magnified about 1000 times. Moreover, the mere substitution of a different lens makes it possible to magnify as much more as may be desired, certainly up to 5000 times without much difficulty.

The apparatus used is illustrated diagrammatically in figure 1, page 194, which shows all the essential parts. We took an Edison automatic reproducer, such as is regularly sold with the machines, removed the diaphragm, and fastened to the tracing lever a rigid arm bearing an adjustable plane mirror. A spring, fastened to this arm, holds the sapphire knob of the reproducer lightly but firmly in the furrow of the wax. It should be just stiff enough to raise the weight of the movable plate, on which the lever is mounted, and press the whole snugly up against the carriage frame, in order that not the smallest movement of the knob can be taken up at bearings, but that every movement, small or great, shall be faithfully imparted to the mirror. Then a narrow beam of light reflected from the mirror and focused on moving bromide paper will leave a sinuous trace exactly corresponding to the profile of the bottom of the furrow in the wax. The magnifying power of this arrangement is very great, because the long arm of the lever is a beam of light, and making this arm longer is for the most part a very simple matter. To magnify 400 times by a system of material

---

[1] Transactions of the Royal Society of Edinburgh, 1878.

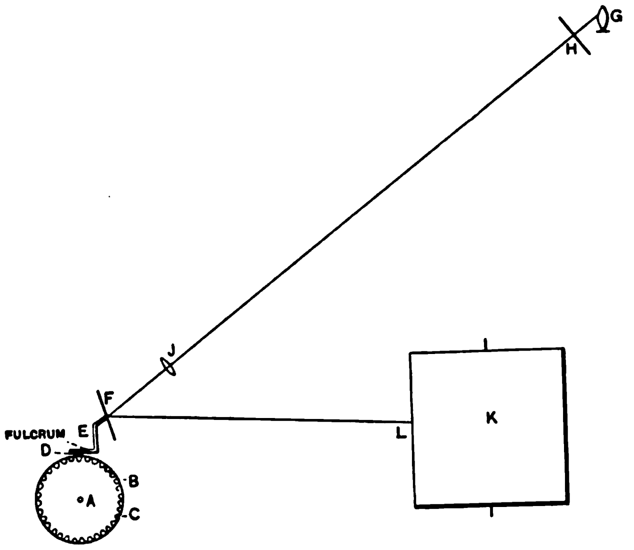


Fig. 1.

A — section of phonograph wax cylinder.
B — surface of the wax.
C — bottom of the furrow, with undulations much exaggerated.
D — sapphire knob of tracer.
E — rigid lever bearing adjustable mirror.
F — adjustable plane mirror.
G — source of light.
H — plate with pin hole through which light passes.
J — convex lens with conjugate foci at *H* and *L*.
K — revolving drum carrying bromide paper.
L — surface of paper on which point of light leaves the sinuous trace.

levers without falsifying the record by various accidental tremors was a matter of great difficulty, whereas by this plan it would be quite easy to magnify as much as 5000 times, if such enlargement should be found desirable. In my machine the distance from the sapphire knob to the fulcrum is about one-eighth of an inch, hence when the other arm is only ten feet we have magnified already nearly 1000 times. Thus far I have been content with this result, but have used a longer arm in a few experiments and found no new difficulties introduced. Making the short arm one-sixteenth of an inch, and working with a beam of light thirty feet in length would obviously magnify more than 5000 times. The chief difficulty would be to secure a fine point of light at so great a focal distance.

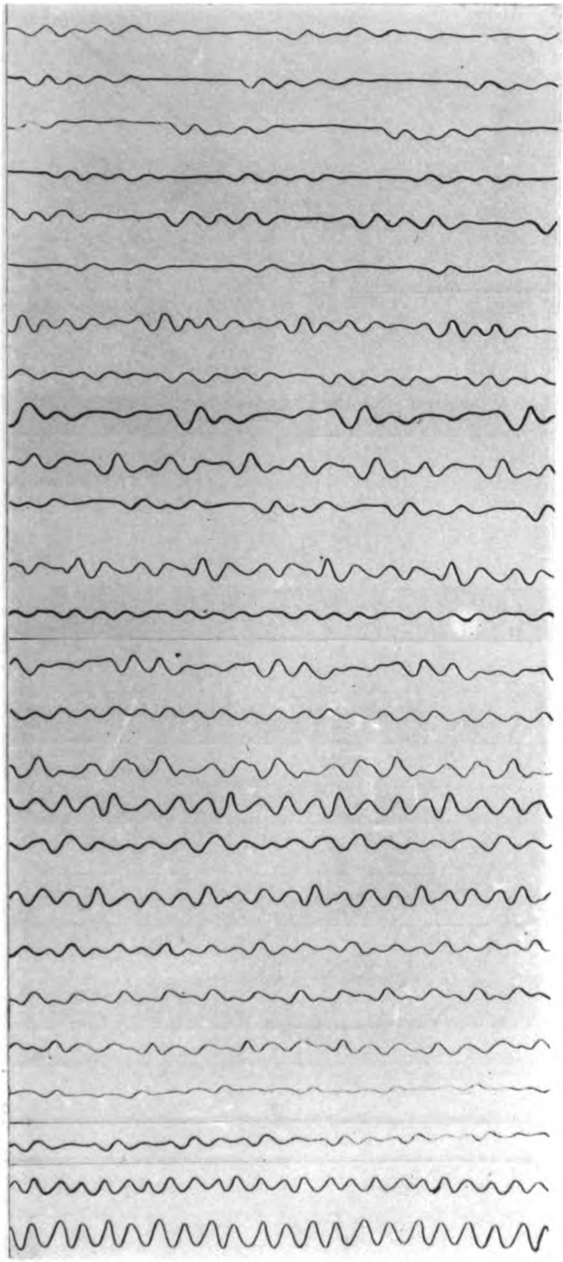
Besides this tracer one must have a good motor to turn the drum on which the bromide paper is carried. This should be adjustable, and as uniform in motion as possible, for if the drum does not revolve uniformly, the abscissas will be hopelessly falsified. Another motor also is needed which will give to the wax cylinder a very slow and uniform motion of rotation. When these preliminaries are arranged and the whole is installed in a room sufficiently dark for photographic work the difficulties of manipulation are not great. I may briefly describe the necessary steps. After the sound which it is desired to study has been sung into the instrument, and the sound-waves are thus recorded on the wax cylinder in the usual way, an ordinary reproducer should be placed in the carriage and a test be made to ascertain whether the instrument gives back the sound with truth and precision. One learns by a little practice to make records which come back with great perfectness of timbre, and this is most important if our results are to have permanent value. A record thus tested by reproduction is now ready to be transcribed. Remove the reproducer and insert in its place the transcriber already explained, adjusting carefully so that the knob runs precisely in the bottom of the furrow. Arrange the light so as to get as small and as perfect a point as possible on the drum. Place the bromide paper on the drum and start the two motors. The point of light will move up and down on the bromide paper, and as the drum revolves a sinuous line is recorded which becomes visible at once when the paper is treated with a developer. The drum should be so adjusted that as it revolves it will move either up or down on its axis so that the trace will not come round into itself. This may be done by hand, but it is better to have an automatic device such as can be arranged in any laboratory. On a strip of paper two feet long and six inches wide we may obtain a sinuous trace in this way of twelve feet or more, according to the amount of axial motion given to the drum with each revolution.

The actual depth of the hollows in the records carried by the wax cylinder is seldom more than 1/2000 of an inch, and generally very much less, so that when ordinates are magnified 1000 times the apparent amplitude as shown in the trace is seldom more and generally less than one-half an inch. An apparent amplitude of one half an inch makes a very loud sound in reproduction, and even when the greatest apparent amplitude is one-tenth of an inch the sound heard in reproduction is very clear and distinct.

With this apparatus I have transcribed many records of the principal vowels of our spoken English, but have as yet made a careful study only of the records of the vowel *a*, that is, the sound heard in such words as *father*, *palm*, *part*, etc. Plate I gives a photograph of some of the curves which I have obtained. They are all waves of the same sound at different pitches, as explained at the side. They were sung by four different voices, 1, 2 and 3 being baritones, and 4 being a boy's voice with soprano register. I have placed at the side the number of vibrations to the second as determined by the pitchpipe used in the present series of experiments. In photographing them they have been reduced nearly three diameters, so as not to occupy too much space. A few specimen waves of the full size as transcribed are shown in Plate II.

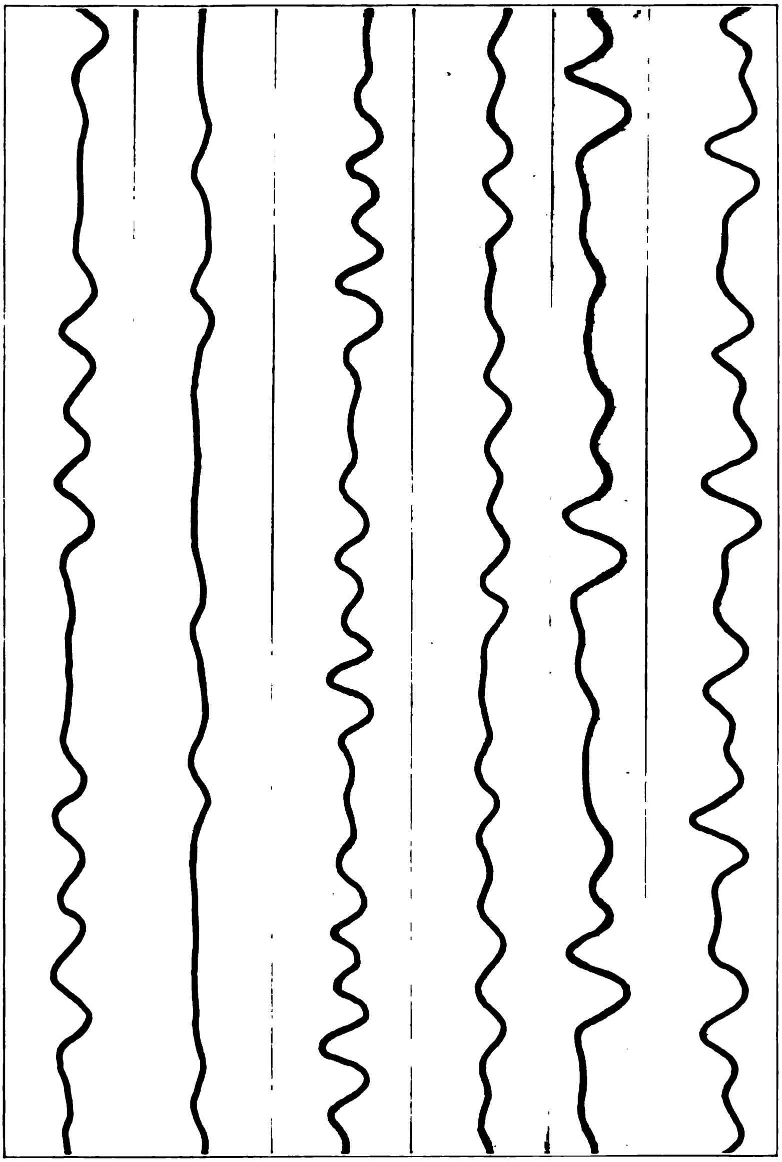
In transcribing the record from the wax cylinder to the bromide paper the cylinder was made to revolve once in about nine minutes, the bromide paper moving at the rate of 23½ inches (the circumference of the drum) in one minute. Now the circumference of the wax cylinder is about six inches, hence it is obvious that abscissas were magnified about 35 times (9 : 6 : : 1 : ⅔ ; ⅔ : 23½ : : 1 : 35¼). Again, since in making the original records the wax cylinder was made to revolve twice a second, it is obvious that a sound of one

CURVES OF THE VOWEL A.



Voice.	Pitch.	Number of waves.
2	113	2
2	144	2+
2	144	2+
2	152	3−
3	171	3+
2	171	3+
1	181	3+
2	202	3+
2	226	3+
1	226	4+
2	226	4+
4	240	4+
2	256	4+
2	256	4+
1	272	5
1	272	5−
4	272	5−
2	288	4+
4	288	5
1	304	5+
1	304	6−
4	320	6−
4	362	6+
4	384	7
4	426	8−
4	540	10+

BEVIER: ACOUSTIC ANALYSIS OF THE VOWELS.



BEVIER: ACOUSTIC ANALYSIS OF THE VOWELS.

This plate reproduces in full size some of the curves shown in Plate I.

second's duration would trace a sinuous line on the bromide paper about 35 feet in length. For a tone at the pitch d (288), the wave-length is therefore about 1½ inches, and at D (144) about 3 inches. In the plate the lines represent therefore about $\frac{1}{68}$ part of a second. These ratios were found to be in practice convenient, but provided that the pitch is in each case carefully registered there is no particular importance in preserving precisely the same relations. The fundamental waves are perfectly distinguishable by the eye alone, and if the pitch is known all the rest is a mere matter of computation. The slow movement of the wax cylinder in transcription is however very important, because we avoid in this way any tremors in the arm which bears the mirror, which otherwise would falsify the record. In the present series of experiments I have employed a speed less than one thousandth of the original velocity, and for some vowels of high resonance pitch (especially i, as in *pique*) I should be inclined to reduce the speed still more. Though the necessity for such slow motion makes experimentation somewhat laborious, since it requires 17 minutes to transcribe a sound of one second's duration, still the necessary preliminaries are so much more exacting of time that an hour more or less in actual transcription does not make much difference.

In one further detail my experience may be of use to other investigators. A cylinder which it is proposed to use in such studies must be carefully planed beforehand. Inequalities which are of no consequence in its ordinary use show as mountainous irregularities when the ordinates are magnified one thousand times. After a cylinder has been planed so that inequalities of this sort are negligible, the desired record should be made and transcribed with as little delay as possible, for the wax is quite sensitive to changes of temperature, and after being left over night is noticeably less regular than when first planed. So, too, after planing it must not be removed from the mandrel and replaced before being used, for it is impossible to replace it so exactly in its former position, but that it will when replaced show again more or less eccentricity.

The waves obtained, specimens of which are shown in Plates I. and II., were subjected to the usual Fourier analysis. In the longest waves, or waves of lowest pitch, 36 ordinates were measured, and the overtones up to the 16th were computed. For the shortest 12 measurements, giving the first four overtones, were found to be sufficient. Intermediate waves were measured in 20 or 24 places, according to their pitch. In no case were any overtones higher than the 10th of large enough amplitude to merit consideration. The subjoined table gives some specimen analyses running from A_1# (with 113 vibrations to the second according to the pitch-pipe used) to d′ (with 576 vibrations to the second). They represent four different voices, and establish with my other material, I believe, a fairly accurate acoustic definition of *a*. For a fuller exposition of these results I refer to an article soon to appear in "Die Neueren Sprachen."

	I.	II	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
A_1#	113	226	339	452	565	678	791	904	1017	1130	1243
2	9.9	15.4	2.5	.8	**11.5**	**16.1**	3.3	8.1	**21.2**	7.9	3.2
D	144	288	432	576	720	864	1008	1152	1296	1440	1584
2	32.9	4.3	1.9	8.5	**14.8**	2.1	8.8	**13.5**	7.2	1.3	4.7
F	171	342	513	684	855	1026	1197	1368	1539	1710	1881
3	5.6	4.8	2.9	**17.5**	5.0	**33.1**	**19.9**	2.1	6.1	1.9	1.1
A#	226	452	678	904	1130	1356	1582	1808	2034	2260	2486
1	9.2	3.6	**34.6**	8.0	**31.0**	2.6	6.2	1.4	.8	1.2	1.4
d	288	576	864	1152	1440	1728	2016	2304			
1	5.4	**18.2**	8.0	**57.3**	3.0	1.3	3.2	3.6			
f#	362	724	1086	1448	1810	2172	2534	2896			
4	9.5	**14.3**	**42.9**	**16.5**	9.5	2.5	3.5	1.3			
a	426	852	1278	1704	2130						
4	5.7	**16.5**	**64.9**	6.0	6.9						
d′	576	1152	1728	2304	2880						
4	21.4	**68.0**	4.0	5.6	1.0						

N B. In this table the fundamental is marked I, the first overtone II, the second III, etc., following the common, but somewhat misleading practice. I have not thought it worth while to adopt Ellis's "partial tone" instead of the familiar "overtone."

It will be sufficient here to describe briefly the conclusions to which I have arrived. The vowel *a* at any pitch, and pronounced by any voice, contains the following elements:

1st, The fundamental, with the first two or three overtones, *i. e.*, the octave above the fundamental, the fifth, and the second octave.

The fundamental varies greatly in relative amplitude for reasons which I do not as yet attempt to formulate. The overtones are generally very weak, unless reinforced by the mouth resonance as set forth below.

2d, and most characteristic for the identity of the vowel; the overtone or overtones whose frequencies of vibration chance to fall between 1000 and 1300 vibrations to the second, the maximum seeming to lie at about 1150. This is the main resonance of the mouth when formed to utter this vowel, and remains remarkably constant no matter what the fundamental pitch may be. If the fundamental has 144 vibrations to the second overtones VII., VIII. and IX., with frequencies of 1008, 1152, and 1296 respectively, will all be present, but VIII., with 1152 vibrations to the second, in much the largest amplitude. If the fundamental has 320 vibrations to the second overtones III. and IV. will be present, with 960 and 1280 vibrations to the second respectively, but IV. will be much the more prominent, III. being very weak since it falls below the region of strong resonance.

3d. The overtone or overtones whose frequencies of vibration chance to fall between 575 and 800 vibrations to the second for men's voices, with a maximum at about 675, or between 675 and 900 with a maximum at about 800, for the voices of women and children. This is presumably the resonance of mouth and throat cavities resounding as one vessel. It is not nearly as constant as the main resonance region explained above, and seems to vary between individuals and classes of voices far more than the other. It is however very important and at one pitch (A♯, = 226) has an amplitude (not however an energy) a little greater even than the main resonance. If the fundamental has 144 vibrations to the second we shall find accordingly overtones IV. and V. present whose frequencies are 576 and 720 respectively, but V. being nearer the point of maximum resonance in this region will have the greater amplitude. At A♯ (= 226) we shall find overtone III., with 678 vibrations to the second very prominent. My analyses reveal for the vowel *a* no other region of resonance except these two.

I subjoin a tentative chart (Fig. 2) representing the resonance curve for the *a*-position of the man's mouth. In this I have made the frequency of vibration the abscissa and the amplitude of vibration the ordinate. Were this based on a long series of observations to eliminate individual and accidental elements we could thus plot a curve which could be taken as the standard resonance curve characteristic of the vowel. It would be practically an objective definition of the vowel, deviations from which in individual cases would be such as would not affect materially the character of the vowel.

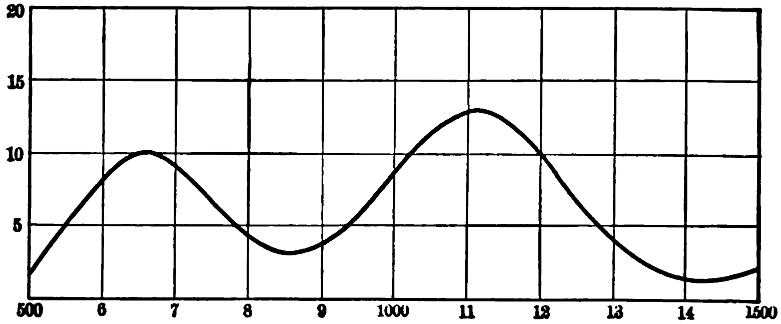


Fig. 2.

The above curve (Fig. 2) is drawn to represent simply amplitude percentages. The "energy" curve, in which ordinates are proportionate to the product of the squares of amplitudes and the squares of frequencies (E varries as a^2n^2) is perhaps even more significant. A tentative energy curve follows:

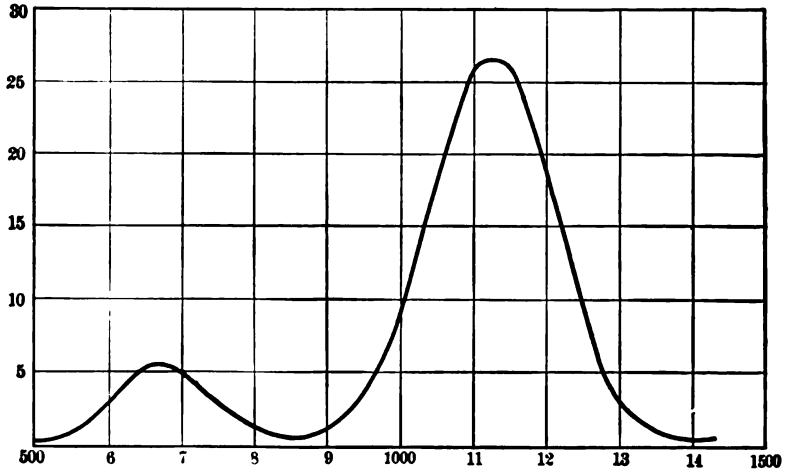


Fig. 3.

Many important questions arise which I do not as yet attempt to answer. A preliminary discussion of some of them will be found in the article to which I have already alluded. My main purpose here is to call attention to a means of experimentation which I am convinced will prove of great use in further researches.

A word as to the mathematical analysis by means of Fourier's theorem may be helpful. It is well to use the average ordinates obtained from a succession of adjacent waves. I say adjacent waves because no matter how steady the voice has been the waves are apt to change slightly in character as the sound proceeds, and though adjacent waves are often, so far as the eye can detect, perfectly similar, it is rarely possible to obtain a sequence of more than a dozen or so which are to the eye exactly alike. Changes of pitch, timbre or intensity so slight as to escape the notice of the ear cause very noticeable modifications in the wave-form, and make it illegitimate to average the measurements of any save adjacent waves. As the succession of waves is examined it will generally be seen that a change is in progress which, though slow, results in the course of a small fraction of a second even in quite a marked difference. An analysis at two different points of the same utterance gives quite a different succession of measurements for ordinates, but the results of a mathematical computation are very similar. Before this matter can be really settled it will be necessary to analyze each wave of a series from beginning to end of an individual utterance. A sufficient number of such computations will probably throw light on this very interesting and difficult subject.

After the necessary measurements have been obtained the analysis is made by means of the familiar formulæ derived from Fourier's theorem, but the labor, particularly when it is necessary to use as many as 36 ordinates, is by no means small. I am indebted to my colleague, Robert W. Prentiss, M.S., Professor of Mathematics in Rutgers College, for a method of arranging the formulæ which has greatly abridged the labor. He is preparing an explanation of his arrangement which will be published shortly.

NOTE ON THE VARIATION OF THE SPECIFIC HEAT OF WATER BETWEEN 0° AND 100°C.

By H. L. Callendar and H. T. Barnes.

THE present note which it is thought would be of interest to many of the readers of the Physical Review is, with a few changes made to introduce later results, almost a reprint of the report communicated by the authors at the meeting of the British Association at Dover (Sept., 1899). In 1897, during the Toronto meeting, a preliminary note was communicated to the same society, in which the authors described briefly a new method for determining the specific heat of a liquid in terms of the international electrical units. At that time the method had only been roughly applied to the cases of water and mercury. The preliminary apparatus, however, was exhibited to several members of Section A on the occasion of their visit to McGill University. One of the main objects of the work was the determination of the variation of the specific heat of water over the range 0° to 100°C. for which the method is peculiarly suited. Shortly after the commencement of the work one of the authors (H. L. C.) was called to London to fill the Quain Chair of Physics in University College, leaving the work in the sole charge of H. T. B.

The general principle of the method, and the construction of the apparatus will be readily understood by reference to the diagram of the continuous-flow electric calorimeter given in Fig. 1. A steady current of water flowing through a fine tube, *AB*, is heated by a steady electric current in a central conductor of platinum. The steady difference of temperature between the inflowing and outflowing water is observed by means of a differential pair of platinum thermometers at either end. The bulbs of these thermometers are surrounded by thick copper tubes, which by their conductivity serve to equalize the temperature, and to prevent the generation of heat by the current in the neighborhood of the bulbs of the thermometers. The leads CC serve for the introduction of the current, and the leads PP, which are carefully insulated, serve for the measurement of the difference of potential on the central conductor. The flow tube is constructed of glass, and is sealed at either end, at some distance beyond the bulbs of the thermometers, into a glass vacuum jacket, the function of which is to diminish as much as possible the external heat loss. The whole is enclosed in an external copper jacket (not shown), containing water in rapid circulation at a constant temperature maintained by means of a very delicate electric regulator.

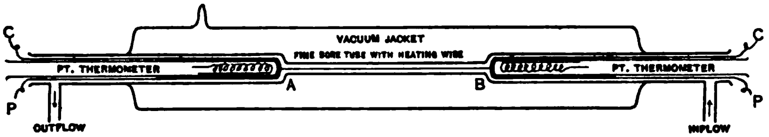


Fig. 1.

The general equation of the method may be stated in the following form:

$$JMd\theta + H = ECt.$$

The difference of potential E on the conductor is measured in terms of the Clark cell by means of a Thomson-Varley slide potentiometer, very accurately constructed and calibrated. The current C is also measured by observing the difference of potential on a standardized resistance R included in the circuit.

The Clark cells employed in this work were "crystal" cells [1] of the B. O. T. pattern and hermetically sealed cells. They were kept immersed in a regulated water bath at a constant temperature, near 15° C., and have maintained their relative differences constant to a few parts in 100,000 since they were made in 1895.

The resistance R consists of two one-ohm coils in parallel, each made of four bare platinum-silver wires in parallel wound on mica frames and immersed in paraffin oil at a constant temperature. The coils were annealed at a red heat after winding on the mica frames, and are not appreciably heated by the passage of the electric currents employed in the work. The two one ohms are repeatedly standardized by comparison with an oil immersed certified one ohm manganin standard of the English Electrical Standards Committee, and have not changed their resistance since being made by as much as 5 parts in 100,000.

---

[1] H. L. Callendar and H. T. Barnes, Proc. Roy. Soc., 62, 117 (1897).

The time of flow *t* of the mass of water, *M*, was generally about fifteen minutes, and was recorded automatically on an electric chronograph reading to .01 second, on which the seconds were marked by a standardized clock.

The letter *J* stands for the number of joules in one calorie at a temperature which is the mean of the range $d\theta$, through which the water is heated.

The mass of water, *M*, was generally from 400 to 600 grammes. After passing through a cooler, it was collected and weighed in a tared flask in such a manner as to obviate all possible loss by evaporation.

The range of temperature was generally about 8° in the series of experiments on the variation of *J*, but other ranges from 1° to 12° were tried to test the theory of the method. The thermometers were read to the ten-thousandth part of a degree, and the difference was probably in all cases accurate to .001° C. This order of accuracy could not have been possible with mercury thermometers under the conditions of the experiments.

The external loss of heat, *H*, was very small, owing to the perfection and constancy of the vacuum attainable in the sealed glass jacket. It was determined and eliminated by adjusting the electric current so as to secure the same rise of temperature, $d\theta$, for widely different values of the water-flow.

The great value of the continuous-flow method as compared with the more common method in which a constant mass of water at a uniform temperature is heated in a calorimeter, the temperature of which is changing continuously, is that in this method there is practically no change of temperature in any part of the apparatus during the experiment. There is no correction required for the thermal capacity of the calorimeter; the external heat loss is more regular and certain, and there is no error due to the lag of the thermometers. Another distinct advantage is that the steadiness of the conditions permits the attainment of the highest degree of accuracy in the instrumental readings.

In work of this nature it is recognized as being of the utmost importance to be able to detect and eliminate constant errors by varying the conditions through as wide a range as possible. In addition to varying the electric current, the water-flow, and the range of temperature, it is possible with comparatively little trouble to alter the form and resistance of the central heating conductor, and to change the glass calorimeter for one with a different degree of vacuum, or a different bore for the flow tube. Several different calorimeters were thus employed, and the agreement of the results, on reduction, affords a very satisfactory test of the accuracy of the method.

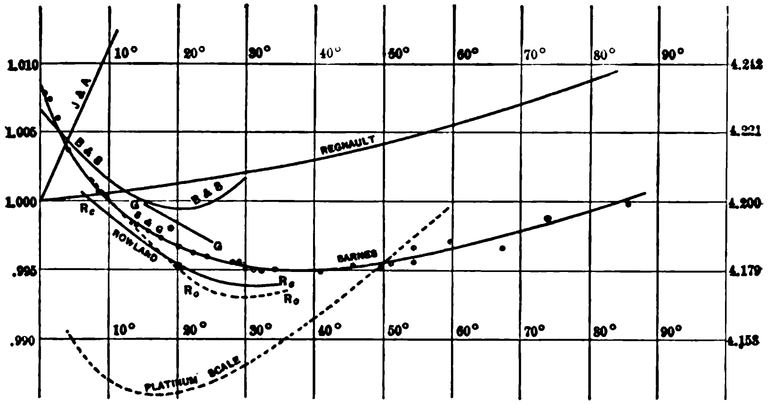


Fig. 2.

Variation of the Specific Heat of Water between 0° and 100° C.

J. & A. Jamin and Amaury, 1870.
B. & S. Bartoli and Stracciati, 1891.
R_o. Rowland's Original Values, 1880.
R_c. Rowland's Corrected Values, 1898.

G. Griffiths, 1893. S. & G. Shuster and Gannon, 1893. ⊙ Authors' Values, 1899.	Lord Rayleigh's Value for the Clark Cell, Assumed.
* Authors' at 20.°2 C.	Kahle's and Carhart's Identical Clark Cell Values, Assumed.

The general results of the investigation, so far as it is possible to give them for publication at present, may be seen by reference to the plot given in Fig. 2, which includes also the results of other observers, to the same scale. The curve marked Regnault represents the well-known formula of Regnault which has been adopted as the basis of much calorimetric work. This formula was confessedly approximate, and was deduced from experiments on mixing water at high temperatures with water at 15° C. The method could not be expected to give any information with regard to the variation of the specific heat at ordinary temperatures. The experiments of Jamin and Amaury (J. and A.), by the method of electric heating, gave a very rapid increase of the specific heat at low temperatures, but the science of electrical measurement, and the difficulties of the electrical method, were not at that date sufficiently known to render the results of any value.

Rowland's original curve (R_0) is shown by the dotted line, and with it his corrected values[1] (R_c) are given by the smooth line. His original results, obtained by Joule's method, were expressed in terms of his own air thermometer, and showed, for the first time, how rapidly the specific heat of water diminishes as the temperature rises from 0° to 30° C. The corrected values are those obtained by reducing his thermometers to the Paris scale. The effect of this correction is to lower the temperature at which the specific heat is 4.200 joules from 10° to 7° C. and to diminish the temperature coefficient. In the plot in Fig. 2 the right hand margin gives the scale of joules in absolute measure.

The experiments of Bartoli and Stracciati (B. and S.) were made by the method of mixtures, and are expressed in terms of a thermal unit at 15° C. Their curve bears a general resemblance to that of Rowland, but shows a minimum point at 20° C. The errors and limitations of this method are well known, and it is difficult to suppose it capable of any high degree of accuracy, or to think otherwise than that the excessive lowering of the minimum point is due to some error not eradicated by mere repetition of similar experiments.

The experiments of Griffiths (G.) between 15° and 25° were made by observing the rate of rise of temperature of a mass of water heated by an electric current. His work threw a flood of light on the difficulties of electric calorimetry, and explained the failure of previous observers to obtain satisfactory results by this method. Over the range of his experiments he found approximately the same rate of diminution of the specific heat as that given by Rowland.

---

[1] William S. Day, PHYSICAL REVIEW, 7, 193 (1898).

The curve marked by circled dots in the figure, extending from 0° to 86° C. represents the results so far obtained in the present investigation. The points represented are samples of single observations with different calorimeters, and give an idea of the order of accuracy attainable by this method. The accuracy of course diminishes as the temperature rises, owing to the greater difficulty of obtaining satisfactory regulation in the water-jacket at the higher points.

The curve shows clearly that the minimum point occurs at 40° instead of 30°. The experiments of Rowland did not extend sufficiently beyond his minimum point to obtain a satisfactory determination. The value of the specific heat could not be determined by his method with the same accuracy at the extremities of the range as in the middle, and all the probable errors of the method would be greatly increased as the temperature of the calorimeter was raised above its surroundings. In particular, the corrections and changes of zero of the mercury thermometers, and the rate of external heat loss, would be excessive at the higher points. In the present work, however, there are no thermometric difficulties of this nature, owing to the use of platinum thermometers, and the external heat loss increases very little as the temperature is raised, because the external water-jacket is always at the same temperature as the inflowing water, so that the mean excess of temperature is always nearly the same. Another indication that the temperature of minimum specific heat is not far below the middle of the range is given by the experiments of Regnault, and more recently by those of Reynolds and Moorby, on the mean specific of water between 0° and 100° C. Their results by entirely different methods agree in showing that the mean specific heat over the whole range does not greatly exceed the value at 20° C.

The present work shows that the specific heat of water approaches an exceedingly high value at 0°. The observations at 0.6°, 1.5°, 2° and 4° were obtained by using rises of temperature of

1°, 3°, 4° and 8° with the inflow water nearly at 0°. This rapid increase, shown by these points, was foreseen by Rowland, but not shown in his work.

It is interesting to compare the absolute values of the specific heat deduced by the electrical method with those of Rowland by the mechanical method. For this purpose the authors' results, and those of Griffiths (G.), and of Shuster and Gannon (S. and G.), have been reduced to joules on the assumption that the absolute value of the E. M. F. of the Clark cell is that found by Glazebrook and Skinner, assuming Lord Rayleigh's value of the electro-chemical equivalent of silver, and taking the international ohm as correct. It has been pointed out that the results of Griffiths would be brought into harmony with those of Rowland by supposing that the true E. M. F. of the Clark cells employed was about 2 millivolts lower, or one part in 700. The authors' results, however, lie about midway between those of Rowland and Griffiths, and would require a correction of only 1 millivolt, if the whole of the difference were to be ascribed to the Clark cell. It is not at all likely that the cells used in this work can have exceeded the B. O. T. standard by so much as 1 millivolt, or that the resistance standards can have been incorrect by as much as one part in 700. It is most likely that both the Clark cells and the resistance standards agreed with those used by Griffiths to within one or two parts in 10,000 and that the difference of the results is mainly to be attributed to the radical difference in the methods of calorimetry. In view of the recent determinations of the absolute value of the Clark cell made by Kahle and later by Carhart and Guthe, the authors' value at 20° C. is reduced assuming the new value of the Clark cell, *i. e.*, 1.4330 volts. This has the effect of lowering the specific heat and causing it to almost exactly coincide with Rowland's corrected value at the same temperature. The complete variation curve would then be lowered the same amount had the new Clark cell value been used in its deduction.

These and similar questions relating to the absolute values of the standards used do not affect the accuracy of the relative results as regards the variation of the specific heat of water with temperature. The relative results are regarded by the authors as being as accurate as the present apparatus is capable of affording. By far the most important consideration affecting the form of the curve is the particular thermometric scale to which the results are reduced. If, for instance, the results were expressed in terms of the platinum scale, which differs from the absolute scale by only 0.38° C. at 50° C., when the divergence is a maximum, the curve would be that represented by the dotted line in the figure. The authors' variation curve is deduced from this by the usual parabolic formula, which gives results in practical agreement with the Paris scale.

Since the discussion of the thermal unit introduced by Griffiths at the British Association meeting of 1895, and partly in consequence of the general interest excited by that discussion, so many new facts have been brought out, and so much experience has been gained of the practical effect of the proposals then made, that it appears desirable to discuss more fully the bearing of the present work on the general question of the relation between the various thermal units.

Dieterici[1] made a determination of the mean specific heat in terms of the electrical units by means of a Bunsen ice-calorimeter. His result gives 4.233 joules as the value of the mean specific heat of water in absolute measure between 0° and 100° C.

Winkelmann[2] endeavored to connect this result with Rowland's at low temperatures by assuming a parabolic formula for the mode of variation, and taking the minimum value at 30.6° C. to be 0.9898 of the value at 0° C. This formula makes the ratio of the mean specific heat between 0° and 100° C. to the specific heat at 20° C. to be 1.0120. According to Regnault's formula the same ratio would be 1.0038. If we take Rowland's corrected value at 20° C. as 4.181 joules, the mean value between 0° and 100° would be 4.197 joules according to Regnault, but 4.233 joules according to Winkelmann. The latter gives a remarkable coincidence with Dieterici, in consequence of which the formula of Winkelmann has been frequently quoted and employed in physical investigation. It must be remarked, however, that Rowland's curve is not even approximately parabolic and that the range covered by his observations is hardly sufficient to justify this method of treatment. It must also be observed that the values given by Winkelmann's formula for the specific heat in the neighborhood of 100°, and still more at higher temperatures, are so large that they cannot possibly be reconciled with the experiments of Regnault and other good observers.

---

[1] Wied. Ann., 33, 417 (1888).

[2] Handbook of Physics, Vol. II., Pt. 2, p. 338.

Griffiths[1] came to the conclusion from a comparison of his experiments on the latent heat of evaporation of water at 30° C. and 40° C. with those of Dieterici at 0° C. expressed in terms of the mean specific heat, and with those of Regnault on the total heat or steam at 100° C. that the mean specific heat must be very nearly identical with the specific heat at 15° C., although Regnault's direct experiments made the ratio from 0.5 per cent. to 1 per cent. larger. At his suggestion Prof. Joly performed the inverse experiment of determining the mean specific heat between 12° and 100° with his steam calorimeter in terms of the latent heat of steam at 100° taken as 536.63 times the thermal unit at 15° C. The result of this experiment was to make the mean specific heat appear nearly 0.5 per cent. smaller than the specific heat at 15° C. If we suppose that the inversion of the experiment would tend to reverse the error of the original determination of the latent heat, the result would appear to be strongly in support of Griffith's contention.

Peabody, in the preface of his "Tables of the Properties of Saturated Steam" (1896), as a result of a careful discussion of Rowland's and Regnault's experiments, adopts Rowland's values from 0° to 40°, and expresses his results in terms of the mean specific heat between 15° and 20°. He finds that Regnault's experiments may be sufficiently represented in terms of this unit by assuming the specific heat to be constant and equal to 1.003 between the limits 45° and 155°, and constant and equal to 1.046 between the limits 155° and 200° C. This assumption would make the mean specific heat between 0° and 100° have the value 1.0044 in terms of the specific heat at 17.5° C., or the value 1.0056 in terms of the specific heat at 20° C., assuming Rowland's coefficient of diminution. The general effect of these changes is to make the tables agree fairly well throughout with Regnault's experiments, but the method can only be justified on the ground of expediency, and can hardly be regarded as a satisfactory reconciliation of conflicting evidence on account of the assumed discontinuities in the specific heat.

---

[1] Phil. Trans., 7, 318 (1895).

Shaw[1] gives a similar reduction of Regnault's experiments by means of Rowland's original table, but tabulates only the total heat in joules at each point between 100° and 180° C. His reduction shows a similar flattening of the curve between 100° and 150°, as compared with Regnault's formula. This may be a physical fact, but might also be explained by supposing that the earlier experiments at 108° to 120° were about 0.4 per cent. too high. Shaw's reduction expressed in terms of a thermal unit at 20° C. is given for comparison in the table.

Recently a direct determination of the mean specific heat in terms of the mechanical units has been made by Reynolds and Moorby on a large scale with Reynolds' break and a steam engine. Their results expressed in absolute measure is 4.1832 joules, and is entitled to very great weight on account of the minute accuracy of the measurements, and the full discussion of possible sources of error. It exceeds the value found by Rowland at 20°C. by only one part in two thousand, but is no less than 1.20 per cent. smaller than the mean value found by Dieterici. This is a discrepancy far too large to be explained by any uncertainty in the values of the electrical units.

Accepting the trustworthy measurements of Reynolds and Moorby it is clear that the minima of specific heat at 20° and 30° indicated by the work of Bartoli and Stracciati and of Rowland respectively, must be due to some source of error in their methods, and that all formulæ hitherto proposed for the mode of variation of the specific heat between 0° and 100° must be abandoned.

It is possible, however, to deduce a more satisfactory comparison of the results of Rowland with those of Reynolds and Moorby by means of the present series of experiments, on account of their great range, and the close agreement of the individual observations. Neglecting for the present the rapid change of the specific heat in the immediate neighborhood of 0° C., it may be observed that all the authors' observations between 10° and 60° (with the exception of one at 55°) are represented to about one part in 5,000 (*i. e.*, within the limits of agreement of the observations with different calorimeters at any one point) in terms of the minimum value S_{40} at 40° C., by the simple formula

---

[1] B. A. Report, 1896, p. 162.

$$S_t = S_{40}(1 + .0000045(t - 40°)^2).$$

which gives for the mean specific heat between 0° and $t°$ the formula

$$S_0^t = S_{40}(1.0072 - .00018t + .0000015t^2).$$

If this formula could be assumed to hold beyond these limits over the whole range 0° to 100, the ratio of the mean specific heat between 0° and 100° to the specific heat at 20° would be 1.0024. Assuming Rowland's 4.181 joules at 20°, this ratio would give the value 4.191 joules for the mean specific heat, a result obviously in excess of Reynolds' and Moorby's 4.183 joules.

It is evident from the observations at higher temperatures that the variation curve is not symmetrical, but flatter between 60° and 100°. The rate of change of the specific heat at 100° as given by the above formula, if extrapolated, is more than twice as great as that given by Regnault, and at 200° about four times as great. The experiments of Regnault apply particularly to this portion of the range, for which they have remained the standard, and have been so universally adopted. It would be desirable to retain his formula for the present, with such change as would make it fit the present experiments. It will be readily seen by reference to the figure that Regnault's curve is very nearly parallel to the authors' curve between 40° C. and 86° C. The two curves can then be very accurately fitted over this range by simply subtracting a constant quantity from the values given by Regnault's formula. This method leads to results, which require the simplest modification of existing tables.

Adopting the authors' formula between 10° and 60°, and the corrected formula of Regnault from 60° to 100°, the ratio of the mean specific heat between 0° and 100° to the specific heat at 20° is 1.0014. Taking Rowland's value as 4.181 joules at 20°, this ratio would give 4.1868 for the mean specific heat, which exceeds the value found by Reynolds and Moorby by less than one part in a thousand—a discrepancy so small as to be within the limits of

Table of the Specific Heat of Water.

Range 0° to 60°C.

FORMULA.

$$S_t = 0.9982 + 0.0000045\,(t - 40°)^2.$$

t°C.	Joules.	S_t	S_{40}^t	h Authors' Formula.	Rowland (reduced).
0	4.203	1.0054	—	—	—
5	4.196	1.0037	1.0045	5.023	5.023
10	4.190	1.0022	1.0037	10.037	10.044
15	4.185	1.0010	1.0030	15.045	15.054
20	4.181	1.0000	1.0024	20.048	20.057
25	4.178	0.9992	1.0018	25.045	25.053
30	4.176	0.9987	1.0013	30.039	30.043
35	4.174	0.9983	1.0009	35.032	35.039
40	4.174	0.9982	1.0006	40.024	Peabody
45	4.174	0.9983	1.0003	45.016	45.000
50	4.176	0.9987	1.0001	50.008	50.040
55	4.178	0.9992	1.0000	55.002	55.080
60	4.181	1.0000	1.0000	60.000	60.120

Range 60° to 220°C. Regnault (Corrected).

Formula $S_t = 0.9944 + 0.00004t + 0.0000009t^2$.

60	4.181	1.0000	1.0000	60.000	60.12	
65	4.184	1.0008	1.0000	65.002	65.16	
70	4.188	1.0016	1.0001	70.008	70.20	
75	4.191	1.0024	1.0002	75.018	75.24	
80	4.195	1.0033	1.0004	80.032	80.28	
85	4.199	1.0043	1.0006	85.051	85.32	
90	4.203	1.0053	1.0008	90.075	90.36	
95	4.207	1.0063	1.0011	95.105	95.40	
100	4.212	1.0074	1.0014	100.138	100.44	
110	4.222	1.0097	1.0020	110.22	110.67	Shaw.
120	4.232	1.0121	1.0028	120.33	120.73	
130	4.243	1.0148	1.0036	130.47	130.80	
140	4.255	1.0176	1.0045	140.63	140.88	
150	4.267	1.0206	1.0055	150.82	151.01	
160	4.281	1.0238	1.0066	161.05	161.20	
170	4.295	1.0272	1.0077	171.31	171.61	
180	4.310	1.0308	1.0089	181.60	182.14	
190	4.326	1.0345	1.0102	191.94	—	
200	4.342	1.0384	1.0115	202.31	—	
210	4.359	1.0425	1.0130	212.72	—	
220	4.377	1.0467	1.0145	223.18	—	

possible error even in the case of these two extremely accurate determinations.

The annexed table has been constructed on the basis of the above considerations and represents, by the authors' formula combined with the corrected formula oi Regnault, the variation of the specific heat of water between 0° and 220° C. The absolute measure at 20° C. given by Rowland, *i. e.*, 4.181 joules, is taken as the basis of column 2. The values of the specific heat are worked out assuming a unit at 20° C. and neglecting the rapid increase below 10° C. The general effect of this table is to diminish the extent of the variation hitherto assumed, but it is believed that the results here tabulated are within the limits of error of all the best determinations. The order of agreement may be inferred from a comparison of the values of *h*, the total heat of the liquid, given in the last two columns. The agreement with Rowland is within 1 in 3,000, between 10° and 40°, and with Regnault within 1 in 1,000 at 160° C. The variations of Regnault's individual observations exceed 5 parts in 1,000.

The values of the total heat *h* are obtained by integrating the specific heat from 0° to *t*° by the formula. By adding below 10° the constant quantity .020 to the value of *h* as given in the table, account is taken of the rapid increase at the freezing point.

In conclusion it may be stated that the work is being rapidly continued to verify the points obtained above 60° C. The observations at the higher points are exceedingly difficult and tedious to obtain but with the present arrangements designed especially with this object in view observations as high as 95° C., will probably be obtained.

MACDONALD PHYSICAL LABORATORY, MCGILL UNIVERSITY, Feb. 3, 1900.

THE MAGNETIC MOLECULAR SUSCEPTIBILITY OF SALTS OF THE IRON GROUP.

By O. Liebknecht and A. P. Wills.[1]

GUSTAV WIEDEMANN was the first to undertake magnetochemical investigations in regard to the behavior of the paramagnetic salts of the metals: chromium, manganese, iron, cobalt, nickel and copper. He used a torsion method with a non-uniform field.[2] He found that the quantity defined by him as the "Molecularmagnetismus" is nearly independent of the solvent, the concentration, and especially of the nature of the anions for a given temperature and cation of a given valency. The differences always exceeded the experimental errors and amount in general to 2 or 3 per cent. Therefore he found a mean value for a given cation and called it the "Atommagnetismus." The atomic magnetisms of the manganous, ferrous, cobaltus and nickelous salts were as $a + 2b$, $a + \frac{3}{2}b$, $a + b$, a, where $b = 1.15a$; that of the ferric salts exceeds that for the ferrous salts by about 15 per cent.

Not long since G. Jäger and St. Meyer[3] published some striking measurements made with a modification of the Quincke manometric method. They drew the conclusion that the atomic magnetisms of manganese, iron, cobalt, nickel are as 6 : 5 : 4 : 2; this agrees with the proportion given above if $a = b$. Their results were obtained from the chlorides, sulphates and nitrates; with manganese, cobalt and nickel, only the bivalent salts could be used; on the other hand the value for iron was taken as the mean of the results for ferric chloride, ferrous sulphate and ferric nitrate.

---

[1] The chief results were communicated to the Physikalische Gesellschaft on the 30th of June, 1899; many results were still outstanding.

[2] G. Wiedemann, Pogg. Ann., 126, p. 1, 1865; 135, p. 177, 1868; Wied. Ann., 5, p. 45, 1878; P. Plessner, Wied. Ann., 39, p. 336, 1890; G. Quincke, Wied. Ann., 24, p. 392, 1885. G. Wiedemann, Lehre von der Elektricität, 2. Aufl. 3, §§ 1266–1291, 1895. J. Koenigsberger, Wied. Ann., 66, p. 698, 1898.

[3] G. Jäger und St. Meyer, Wien. Ber. (2), 106, pp. 594, 623, 1897; 107, p. 5, 1898.

In later articles Jäger and St. Meyer use for ferrous chloride the relative number 3 instead of 5 as above; further, they obtain from chromic chloride and chromic sulphate the number 2.5 for chromium.

In the light of these results it seemed to us desirable to make new determinations using a different method. We have made quantitative determinations upon 36 salts of the five metals of the iron group and of copper. Great stress was laid upon the procuring of the purest material possible, and all salts in which there was danger of rapid decomposition were discarded. In general, we have studied the sufficiently soluble fluorides, chlorides, bromides, iodides, sulphates and nitrates; for the reason just mentioned, it was not possible to study many of the iron and chromium compounds. The accuracy of our final results may vary between 2 and 5 parts in 1,000, according to the method used in the chemical analysis.

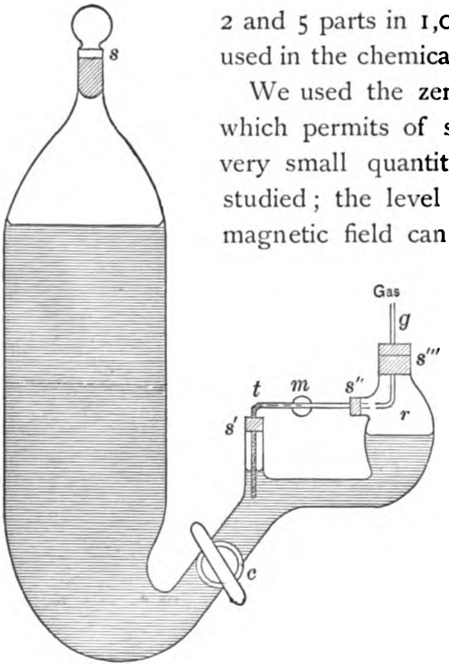


Fig. 1. ¼ natural size.

We used the zero method of Prof. du Bois,[1] which permits of successful manipulation with very small quantities of the substances to be studied; the level of the capillary tube in the magnetic field can be so adjusted that the unmagnetic solution can be prepared with great precision so as to be well within the limits of error in the subsequent chemical analysis. Briefly, the procedure is as follows: A conveniently strong paramagnetic solution is prepared and then diluted with the diamagnetic solvent until the solution is unmagnetic. The practicability of the method is only limited by the susceptibility and solubility of the salt. Sometimes the method is inadmissible on this account as, for example, in the case of potassium chromate and bichromate and of potassium and calcium permanganate. These peculiarly constituted salts are well known to be but weakly paramagnetic; the similarly constituted dry K_2FeO_4 proved to be much more paramagnetic; the alkali solution decomposed so quickly that we were prevented from making a quantitative determination.

---

[1] H. du Bois, Wied. Ann., 35, p. 154, 1888; 65, p. 38, 1898.

As a criterion for the unmagnetic condition of the solution, the behavior of its meniscus in a Quet's[1] capillary tube *t* in a magnetic field was observed (Fig. 1). The large mixing vessel had a capacity of about 600 ccm.; it could be closed by means of a glass stopper *s*, and could be separated from the rest of the apparatus by means of the glass stopcock *c*; the arm connected with the mixing vessel was so formed that fresh capillary tubes could be suitably introduced and fixed by means of the rubber stoppers at *s′* and *s″*. The space beyond the meniscus in the capillary tube and the space *r* could be filled with washed moist illuminating gas.[2] This apparatus was fixed to a suitable stand permitting a rotation about a horizontal axis perpendicular to the plane of the figure. The inclination of the capillary tube could be thereby regulated by means of a suitable adjusting screw; and, moreover, by means of a vigorous rotation of the apparatus about this axis a thorough mixing of the solution could be attained. At *m* is represented the end view of the truncated conical pole piece of the ring electro-magnet.

With a little practice a very short time sufficed to prepare an unmagnetic solution with an accuracy of one part in 1,000, so that upon excitation of the electro-magnet no noticeable motion of the meniscus took place. At each dilution the mixing apparatus was, of course, well shaken and each part well washed with the diluted solution, so that the solution should have in all parts the same concentration as at the meniscus. Any possible local change in the concentration due to magnetic influences would scarcely have time to be effected; furthermore, though such an effect theoretically exists, it has not been unmistakably shown experimentally.[1] In case the capillary is inclined toward the horizontal by a fraction of a degree, it is possible with a field of 40,000 c. g. s. units, to detect a differential susceptibility which does not exceed a hundred thousandth of that of water. On account of the above mentioned limits of error in the chemical analysis, it was useless to try for such accuracy as this, so that as a rule a much less sensitive adjustment was employed.

---

[1] Quet, Comp. Rend., 38, p. 562, 1854; E. Verdet, Oeuvres, 1, p. 199

[2] The susceptibility of illuminating gas was neglected; as far as known it amounts at a pressure of one atmosphere to less than one thousandth of that of water; compare M. Faraday, Exp. Res., 3, p. 502, 1855; G. Quincke, Wied. Ann., 34, p. 445, 1888; A. Toepler u. R. Hennig, Wied. Ann., 34, p. 796, 1888. An absolutely unmagnetic gas can be made after the manner of an unmagnetic solution by mixing a little oxygen with a diamagnetic gas; compare du Bois, l. c., p. 167; of course it is here tacitly supposed that the permeability of a vacuum serves as the unit in the usual conventional way.

The procuring and cleaning of the capillary tubes offered at first some difficulties. Finally the freshest possible tubes from Thüringer glass were used, these being kept under water until required.[2] With these the mobility of the meniscus was satisfactory. Any influence of the breadth of the capillary tube used upon the concentration of the unmagnetic solution contained therein, could not be detected. This furnished, as is easy to see, an indirect proof that the capillary constants of unmagnetic solutions are quite unaltered in the magnetic field.

This independence of the surface tension of magnetic influences seems to obtain in general.

Brunner and Mousson[3] have fruitlessly sought such an action, whereby they seem to have overlooked the well known-Quincke effect of elevation (or depression) in the magnetic field, which, however, W. Wertheim[4] probably observed. Quincke[5] first undertook drop experiments with strong paramagnetic manganese and iron solutions; he found that the weight of a drop was unaltered by a uniform magnetic field and assumed, therefore, the constancy of the surface tension of such solutions. On the other hand, in a non-uniform field between conical pole pieces the drops of manganese chloride as of iron chloride were larger or smaller upon the excitation of the magnetism according as they formed under or above the shortest lines of force.

---

[1] Compare with G. Wiedemann, Electricität, 2, Aufl. 3, § 1205, 1895.

[2] Compare P. Volkmann, Wied. Ann., 66, p. 194, 1898.

[3] C. Brunner u. A. Mousson, Pogg. Ann., 79, p. 141, 1850.

[4] W. Wertheim, Comp. rend., 44, p. 1022, 1857; Pogg. Ann., 102, p. 595, 1857.

[5] G. Quincke, Pogg. Ann., 160, p. 586, 1877; Wied. Ann., 24, p. 375, 1885. Compare also Durchflussbeobachtungen by H. Dufour, Lum. Electr., 23, p. 337, 1887; likewise G. Jäger u. St. Meyer, Wied. Ann., 67, p. 711, 1889.

For a further investigation of these phenomena we constructed an apparatus (Fig. 2) by means of which the formation of the drop could take place at the nozzle *n* which was of about 1 mm. inside breadth, and at the proper pressure and at different rates, and moreover either in a uniform field of about 18,000 c. g. s. units between surface pole pieces, or in a non-uniform field of about 40,000 c. g. s. units between conical pole pieces *M* and *M'* and while the nozzle was placed in the position of greatest field intensity. The vessel *A* was in connection through *S* with the reservoir *B* in which the level of the liquid could be regulated. The space *r*, *r* could be connected through the cock *c* with a pump; the pressure regulated in this manner was read off on the manometer *m*. A number of experiments were carried out with this apparatus and, briefly stated, the results are as follows:

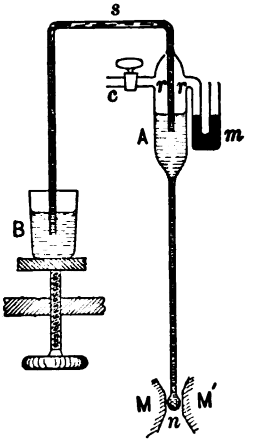


Fig 2. ⅙ natural size.

Paramagnetic solutions show in a uniform field, an increased rate of dropping when the magnetic force upon the liquid column acts downwards; through diminution of the pressure the original rate being again approximately established,[1] the weight of the drop suffers no change. On the other hand, we observe an enlargement of the drop in a non-uniform field; the form of the drop is then changed and depends upon the particular distribution of the field as regards the sense and manner in which it happens. These experiments confirm Quincke's results and conclusions.

Diamagnetic water shows in a uniform field a slower rate of dropping when the magnetic force is directed upwards and, therefore, against the hydrostatic pressure; under proper conditions the dropping may be quite stopped through excitation of the field. If the original rate of dropping be restored through increase of the pressure the weight of the drop suffers no change. In a non-uniform field the weight appeared less with the arrangement used, and more so the less uniform the field.

---

[1] This must be done because, as well known, the rate under ordinary conditions exerts a small secondary influence upon the weight of the drop.

Unmagnetic solutions are not at all influenced, as regards the magnitude of the drop, through excitation of the field if the drops fall in an atmosphere of illuminating gas. Toward air they behave, naturally, like a weakly diamagnetic liquid. This simple criterion has an application in the preparation of unmagnetic solutions; in accuracy this procedure is inferior to the method already described and the one, as a rule, used by us.

THEORY OF THE METHOD.

The extension of our knowledge of magnetic phenomena might be expected, from the first, in a thorough investigation of the thermo-magnetic and magneto-chemical processes. It would seem then to be not superfluous to give in this place, concisely as possible, the elementary conceptions connected with calculations on this subject. The reference of the magnetic moment to the unit of mass, once much employed, has been quite displaced in consequence of the advanced development of the mathematical theory of electromagnetism, because in mathematical physics the reference to the unit of volume is simpler. Professor du Bois[1] has shown that in some cases it is desirable to return to the original method, because the results are in many respects more easily comparable when the variable density is eliminated. Since magnetic processes are essentially molecular it is still better to refer them for stoichiometrical purposes to molecular masses.

In what follows m means molecular weight; M, mass; $\mathfrak{M}$, magnetic moment; $\mathfrak{S}$, specific magnetization, *i. e.*, moment per unit of mass; $\mathfrak{H}$, field intensity; i, specific susceptibility defined by the relation $i = \frac{\mathfrak{S}}{\mathfrak{H}}$.[2] The molecular susceptibility i_m is now defined by the equation

$$i_m = mi = \frac{m\mathfrak{S}}{\mathfrak{H}}.$$

In what follows, the subscripts l, w, s refer respectively to the solution, solvent and salt. On account of the additive properties, first shown by G. Wiedermann, of the quantities here coming into consideration, the following equation obtains for a solution in a uniform field

---

[1] H du Bois, Phil. Mag. (5), 29, p. 293, 1890.

[2] If we designate, as customary, the density with D, the magnetization with $\mathfrak{J}$, the susceptibility according to the ordinary definition with κ, then is $\mathfrak{S} = \frac{\mathfrak{J}}{D}$ and $i = \frac{\kappa}{D}$. Compare J. Koenigsberger, Wied. Ann., 66, p. 698, 1888.

$$\mathfrak{M}_l = \mathfrak{M}_w + \mathfrak{M}_s.$$

The influence of form as well as the therewith connected magnetic interaction of the separate similar or dissimilar volume elements may be with the strongest paramagnetic solutions disregarded since the square of the susceptibility in comparison with unity is quite negligible. In particular for an unmagnetic solution $\mathfrak{M}_l = 0$ therefore $\mathfrak{M}_s = -\mathfrak{M}_w$, or $\mathfrak{S}_s M_s = -\mathfrak{S}_w M_w$. After division by $\mathfrak{H}$ we get

$$(1) \qquad \frac{i_s}{i_w} = -\frac{M_w}{M_s}$$

and the relative molecular susceptibility $\frac{i_m}{i_w}$ of the salt becomes finally

$$(2) \qquad \frac{i_m}{i_w} = -m\frac{M_w}{M_s},$$

i. e., equal to the negative product of the molecular weight into the mass ratio of the solvent and salt. This value of i_m holds not only for the particular concentration of the unmagnetic solution but quite generally;[1] whether, considering the accuracy of our new method, it is quite independent of the kind of solvent can only be decided by a special series of experiments; it changes markedly with the temperature.

Professor du Bois has shown that the behavior of an unmagnetic solution in fields of different intensities permits a conclusion as to the constancy of the susceptibility and has shown this constancy to exist with fields up to 10,000 c. g. s. units.

If the constitution of the unmagnetic solution measured by $\frac{M_w}{M_s}$, is independent of the field intensity, then it follows directly from equation (1) that the relative susceptibility with reference to water $\frac{i_s}{i_w}$ is also independent of the field.

---

[1] According to the results of various investigators; compare with J. Koenigsberger, l. c., p. 708.

[2] H. du Bois, Wied. Ann., 35, p. 155, 1888.

In fact, such a dependency was not found for any of the unmagnetic solutions investigated, and this holds for the extended range of fields between 2,000 and 40,000 c. g. s. units. Now it can be asserted with a probability bordering upon certainty that the variations of the susceptibility of water would not be followed exactly proportionately by those of the different salts. Therefore, the constancy of the absolute susceptibility of the dissolved salt, as of water, within the specified range of fields, follows from the constancy of the susceptibility of the dissolved salt relative to water.

Furthermore, it is very improbable that the susceptibility for weaker fields would be variable.[1] In the results to follow the constant susceptibility of water enters everywhere as a principal factor. We have the following determinations for this factor which have been recalculated so as to refer to a temperature of 18°, and to a vacuum; the susceptibility of air at one atmosphere pressure and 18° was taken as 0.023×10^{-6}.[2]

TABLE I.

	Temperature 18°.	Toward Vacuum.
1885	G. Quincke (most accurate value)	$\kappa = -0.797 \times 10^{-6}$
1888	H. du Bois	-0.831 "
1892	S. Henrichsen	-0.727 "
1895	P. Curie (mean value)	-0.788 "
1896	J. S. Townsend	-0.735 "
1898	J. Koenigsberger	-0.785 "
1899	G. Jäger–St. Meyer (mean value)	-0.645 "
	Mean value	$\kappa = -0.758 \times 10^{-6}$

Since $D_{18^\circ} = 0.9987$, i_w becomes -0.759×10^{-6}

In consideration of the obvious uncertainty of the value of the fundamental constant i_w and its temperature coefficient we have for the present designated it by x and taken the molecular susceptibility relative to it; and then in order to obtain approximate absolute values we have taken provisionally $x = -0.75 . 10^{-6}$. A thorough discussion of the above values and further accurate new determinations would be desirable. So soon as these can be carried through with the accuracy with which the relative values $\frac{i_m}{x}$ are determined, the reduction of the latter to absolute numbers may be easily accomplished.

---

[1] Compare Vgl. F. Auerbach in Winkelmann's Handbuch der Physik, 3, 2 Abth., p. 217. Breslau, 1895.

[2] G. Quincke, Wied. Ann., 24, p. 404, 1885; H. du Bois, Wied. Ann., 35, p. 167, 1888; S. Henrichsen, Wied. Ann., 45, p. 53, 1892; P. Curie, Thése Nr. 840, p. 31, Paris, 1895; J. S. Townsend, Proc. Roy. Soc., 60, p. 186, 1896; Beibl. 20, p. 796, 1896; J. Koenigsberger, Wied. Ann., 66, p. 703, 1898; G. Jäger und St. Meyer, Wied. Ann., 67, pp. 427 u. 707, 1899. The old determinations of M. Faraday, E. Becquerel, J. Schuhmeister and the value of Théodore Wahner's which falls so far out are not considered.

The constitution of an unmagnetic solution is variable with the temperature; we have therefore carefully maintained a normal room temperature of 18°C.[1] The unmagnetic solutions prepared according to this are then at higher (lower) temperatures somewhat diamagnetic (paramagnetic) because the temperature coefficient for the salt is always greater than for the water.

In case the latter were accurately known, the former for different salts could easily be determined.[2]

THE CHEMISTRY DETAILS.

Most of the salts were pure preparations from Kahlbaum or Merck, and were, when possible, many times recrystallized before use. Most of the fluorides, bromides and iodides were prepared by one of us from pure carbonates. The nickel salts were procured from Kahlbaum as "cobalt-free" and the cobalt salts as "nickel-free"; the influence of the chemically unrecognizable impurities present cannot make a difference in the molecular susceptibility fonnd of more than 0.5-per-cent. solutions, which especially incline to decomposition, *e. g.*, MnF, MnCl, FeSo, FeCl, FeBr, were usually treated with a free acid; this was without noticeable influence on the results since the susceptibility of the acid added differs little from that of water; the ferrous iodide contained some free iodine. Where a gradual decomposition was to be feared, the solution was analyzed as soon as prepared; quickly decomposable solutions were not investigated, as already remarked; of course, care was taken that no dilution or concentration change should take place after the solution was once prepared. The gravimetric analysis was carried out for the chromium salts by precipitating them with ammonia as hydroxides. The manganese solutions were precipitated with phosphorus-free hydrogen peroxide and ammonia as oxides and weighed as mangano-manganic oxides. The iron salts were determined in part with potassium permanganate by titration and in part precipitated with ammonia as hydroxides. Finally, the cobalt, nickel and copper were electrolytically precipitated, the two first in a neutral solution with ammonium oxalate and the copper in a solution of sulphuric acid containing a little nitric acid.

---

[1] Compare F. Kohlrausch und L. Holborn, Leitvermögen der Elektrolyte, Leipzig, 1898.

[2] In such measurements, an unmagnetic atmosphere should bound the liquid. The marked dependence of the susceptibility of oxygen relative to air upon the temperature, which has already been mentioned, would otherwise bring in a considerable error.

RESULTS.

Our quantitative results are collected into Table II. q designates the valency of the cation of the salt; a the atomic weight; the metals are arranged according to their increasing atomic weights.[1] The position of cobalt and nickel in the series is reversed, however, after mature consideration. For, according to the collective properties in the metallic or amalgamated condition, and in the condition as a paramagnetic compound, cobalt stands nearer to iron than does nickel; likewise for the chemical properties. In the column before the last is given the relative molecular susceptibility x, which is calculated from the equation given above. In the last column the absolute value is given under the provisional assumption $x = -0.75 \times 10^{-6}$ which number may be a number of per cent. false.

---

[1] The atomic weights are from those collected by the German Chemical Society (Chem. Ber., 31, p. 2761, 1898; Wied. Beibl., 23, pp. 69 und 315, 1899, whereby $O = 16.00$). The question as to the atomic weight of cobalt and nickel is still a subject of chemical discussion; compare Cl. Winkler, Zeitsch. anorg. Chem., 17, p. 236, 1898; Th. W. Richards and G. P. Baxter, l. c., 21, 251 and 22, p. 221, 1899; St. Meyer, Wied. Ann., 69, p. 263, 1899, hints at the possibility of an inverted order in the series.

[2] Recalculated according to the collection of W. Ostwald, Stoichiometrie, 2. Aufl., p. 512, Leipsig, 1891.

TABLE II.

Temperature 18°C. $x = -0.75 \times 10^{-6}$

	Name of Salt in Water Solution.	q	Formula.	O = 16.00		$\frac{M_w}{M_s}$	$-\frac{i_m}{x}$	$+i_m$
				a	m			
1	Chromium Sulphate.	3	$Cr_2(SO_4)_3$	52,1	392,38	40,73	2×7990	0,00599
2	Chromium Potass. Alum.	3	$CrK(SO_4)_2$	52,1	283,37	29,63	8400	0,00629
3	Chromium Amm. Alum.	3	$CrNH_4(SO_4)_2$	52,1	262,30	30,77	8070	0,00605
4	Chromium Nitrate.	3	$Cr(NO_3)_3$	52,1	238,22	35,28	8400	0,00629
5	Manganous Fluoride.	2	MnF_2	55,0	93,00	215,71	20060	0,01504
6	Manganous Chloride.	2	$MnCl_2$	55,0	125,90	163,10	20530	0,01540
7	Manganous Bromide.	2	$MnBr_2$	55,0	214.92	94,63	20340	0,01524
8	Manganous Iodide.	2	MnI_2	55,0	308,70	65,49	20220	0,01516
9	Manganous Sulphate.	2	$MnSO_4$	55,0	151,06	133,65	20190	0,01514
10	Manganous Ammon. Sul.	2	$Mn(NH_4)_2(SO_4)_2$	55,0	283,28	71,04	20130	0,01509
11	Manganous Nitrate.	2	$Mn(NO_3)_2$	55,0	179,08	114,37	20480	0,01536
12	Ferrous Iodide.	2	FeI_2	56,0	309,70	55,18	17090	0,01282
13	Ferrous Sulphate.	2	$FeSO_4$	56,0	152,06	111,72	16990	0,01272
14	Ferrous Ammon. Sul.	2	$Fe(NH_4)_2(SO_4)_2$	56,0	284,28	59,42	16890	0,01268
15	Ferric Chloride.	3	$FeCl_3$	56,0	162,35	111,14	18040	0,01356
16	Ferric Bromide.	3	$FeBr_3$	56,0	295,88	66,77	19750	0,01482
17	Ferric Sulphate.	3	$Fe_2(SO_4)_3$	56,0	400,18	101,06	2×20200	0,01515
18	Ferric Ammon. Alum.	3	$Fe(NH_4)(SO_4)_2$	56,0	266,20	75,64	20140	0,01510
19	Ferric Ammon. Oxalate.	3	$Fe(NH_4)(S_2O_4)_2$	56,0	250,08	81,75	20450	0,01533
20	Ferric Nitrate.	3	$Fe(NO_3)_3$	56,0	242,12	74,48	18030	0,01352
21	Cobaltous Fluoride.	2	CoF_2	59,0	97,00	142,60	13830	0,01037
22	Cobaltous Chloride.	2	$CoCl_2$	59,0	129,90	108,21	14060	0,01054
23	Cobaltous Bromide.	2	$CoBr_2$	59,0	218,92	63,13	13820	0,01036
24	Cobaltous Iodide.	2	CoI_2	59,0	312,70	44,08	13790	0,01034
25	Cobaltous Sulphate.	2	$CoSO_4$	59,0	155,06	87,65	13590	0,01019
26	Cobaltous Nitrate.	2	$Co(NO_3)_2$	59,0	183,08	76,62	14030	0,01052
27	Nickelous Fluoride.	2	NiF_2	58,7	96,70	61,50	5950	0,00446
28	Nickelous Chloride.	2	$NiCl_2$	58,7	129,60	46,02	5960	0,00447
29	Nickelous Bromide.	2	$NiBr_2$	58,7	218,62	27,00	5900	0,00442
30	Nickelous Iodide.	2	NiI_2	58,7	312,40	18,62	5820	0,00436
31	Nickelous Sulphate.	2	$NiSO_4$	58,7	154,76	37,39	5790	0,00435
32	Nickelous Nitrate.	2	$Ni(NO_3)_2$	58,7	182,78	32,31	5910	0,00443
33	Cupric Chloride.	2	$CuCl_2$	63,6	134,50	16,33	2200	0,00165
34	Cupric Bromide.	2	$CuBr_2$	63,6	223,52	9,40	2090	0,00157
35	Cupric Sulphate.	2	$CuSO_4$	63,6	159,66	13,72	2190	0,00164
36	Cupric Nitrate.	2	$Cu(NO_3)_2$	63,6	187,68	11,54	2170	0,00163

From the series of numbers in the foregoing table we may draw the following conclusions: The molecular susceptibility shows the well-known increase from chromium to manganese and iron and then a gradual decrease to copper. The four halogen salts could be investigated unobjectionably as bivalent salts only, in the case of manganese, cobalt and nickel; there occurs here again a regular increase from the fluoride to the chloride with a following decrease to the iodide. Simple additive or subtractive properties are obviously not present.

The approximate coincidence of the chlorides and nitrates is striking throughout; in connection with the ferric compounds it is noticeable that the bromides, sulphates, oxalates, give values which are 12 per cent. higher than those which the ferrous compounds furnish; this agrees with the results found by G. Wiedemann. Moreover, the difference among the separate salts amounts to 5 per cent., so that the introduction of a fixed atomic susceptibility for the cation cannot be allowed without considerable arbitrariness. That this quantity exerts by far the chief influence upon the value of the molecular susceptibility has, of course, never been doubted. We have investigated the sulphates of six metals. These are suitable for comparison with earlier observed values and with the proposed algebraic relations which are collected in Table III.

Table III.

Observer.	Cr‴	Mn″	Fe″	Fe‴	Co″	Ni″	Cu″
Liebknecht-Wills (from exp.)	7990	20190	16990	20200	13590	5790	2190
" " (Ni″$=a$)	1.38a	3.49a	2.93a	3.49a	2.35a	a	0.38a
" " ($b=1.25a$)	——	3.50a	2.88a	——	2.25a	a	——
G. Wiedemann (exp. Ni″$=a$)	1.34a	3.29a	2.74a	3.26a	2.26a	a	——
Jäger-Meyer: (Ni″$=a=b$)	——	3a	2.5a	——	2a	a	——

Our values for the sulphates of Mn, Fe, Co and Ni may be approximately represented by Wiedemann's proportion, whereby $b = 1.25a$; of course, only two of the latter numbers are independent since Mn and Ni serve to determine the parameters a and b; putting $a = b$ does not give an agreement with our results. Finally, these are with respect to nickel everywhere larger than those of Wiedemann, also than those of Jäger and Meyer; the departure can only be explained in case the nickel compounds investigated by these observers should have contained cobalt; this, in our judgment, must have been the case.

We wish to thank Dr. W. Meyerhoffer for his kind assistance in carrying out numerous analyses. The investigations were carried out in the laboratory of Professor H. du Bois, to whom our cordial thanks are due.

BERLIN, December, 1899.

GIBBS' THERMODYNAMICAL MODEL.

BY W. P. BOYNTON.

IN Maxwell's Theory of Heat, p. 207, is a drawing showing some of the principal lines on a thermodynamical model, suggested by Professor J. Willard Gibbs, of Yale University. I have been told that Professor Maxwell had two of these models constructed, one of which remained at Cambridge, England, the other being sent to Professor Gibbs at Yale. There is also a copy of this model at Clarke University, Worcester, the only one which I have seen. While there may be others in existence, these are the only ones which I have known of, and I suspect that very few have ever been constructed.

This year, in connection with a course in thermodynamics, two of my pupils are attempting to construct one of these models, but are met by various serious difficulties, which may interest others.

In the diagram of which I speak, which is here reproduced, the directions chosen for the different coördinates are not immediately evident. Even by the aid of the description in the text I have not been able to locate them satisfactorily. In the attempt so to do, I have been guided by the following general considerations. Using Maxwell's notation, in which

$v =$ volume.
$p =$ pressure.
$T =$ absolute temperature.
$e =$ energy.
$\varphi =$ entropy.

The equation relating these quantities is

$$Td\varphi = de + pdv$$

which transposed gives

$$de = Td\varphi - pdv = \frac{\partial e}{\partial \varphi} d\varphi + \frac{\partial e}{\partial v} dv$$

the differential equation of the thermodynamical surface of which the coördinates are the entropy, volume and energy, and whose slope at each point in the principal directions gives the temperature and pressure, by the identities

$$T = \frac{\partial e}{\partial \varphi}, \quad p = - \frac{\partial e}{\partial v}.$$

These are subject to the conditions that T is always positive, and p is usually positive, always so for the gaseous state, usually for the liquid and solid states.

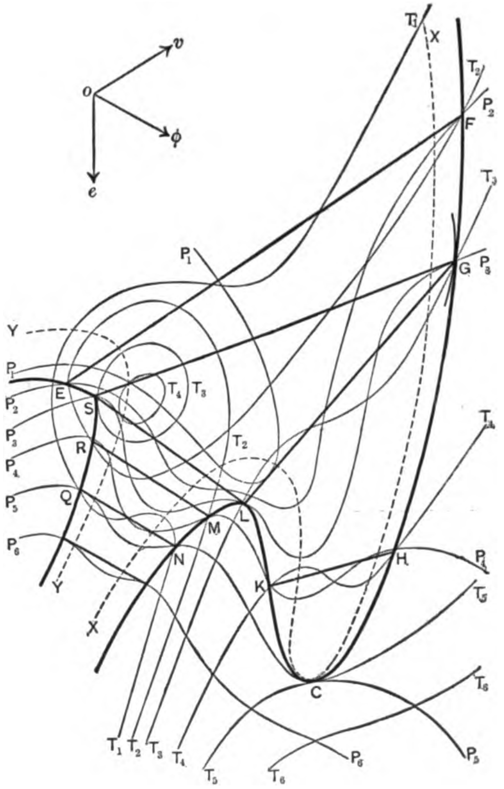


Fig. 1.

If then e is taken vertically downward and v and φ horizontal, passing along a section of the surface by a plane of constant volume, in the direction of increasing entropy the slope will always be downward, and generally convex, as the addition of heat, that is energy to a substance at constant volume increases its entropy, and generally its temperature, never decreasing it. A section by a plane of constant entropy will have a slope in the direction of increasing volume which is in general upward, corresponding to a positive pressure, and in all parts of the model referring to stable states of the substance this will be convex, since increase of volume is then accompanied by decrease of pressure.

I have attempted to determine the choice of coördinates by the properties of the critical state. In the two diagrams the broken line separates the parts representing stable or homogeneous states from parts representing unstable or non-homogeneous states.

In the pressure-volume diagram lines of constant pressure, volume, entropy and temperature are drawn. On the other are drawn lines of constant pressure and temperature, taken from Maxwell. In both diagrams these lines are tangent to the broken line. In Fig. 2, the line $v =$ const. cuts sharply through the broken line. I have attempted to find the behavior of a line of constant entropy in the following way :

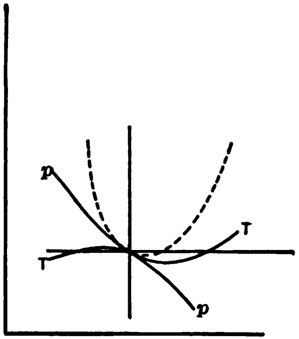


Fig. 2.

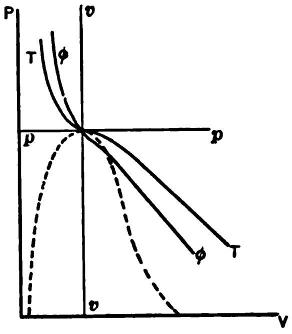


Fig. 3.

For a substance following van der Waals' equation

$$\left(p+\frac{a}{v^2}\right)(v-b)=RT$$

the equation of an isentropic can be shown to be

$$\left(p+\frac{a}{v^2}\right)(v-b)^k = \text{const.}$$

where k is the ratio of the specific heats at constant pressure and constant volume. The slope of this curve is found to be

$$\frac{dp}{dv} = \frac{2a}{v^3} - \frac{k\left(p + \frac{a}{v^2}\right)}{v - b},$$

which becomes, substituting the values of the critical pressure and volume

$$\left(\frac{dp}{dv}\right)_{\text{crit.}} = \frac{2a(1 - k)}{27b^3};$$

which is negative for real positive values of a and b. Hence the isentropic appears also to cut through the broken line, but less sharply.

Still further, we believe that the line of constant volume does not again cut out of this non-homogeneous or unstable area, while the isentropic may. Hence it has seemed to me necessary to consider the vertical line of Fig. 3 a line of constant volume, and the horizontal line an isentropic, while the critical point lies a little to the left of the vertex of the curve, so that the isentropic slightly cuts through the broken line.

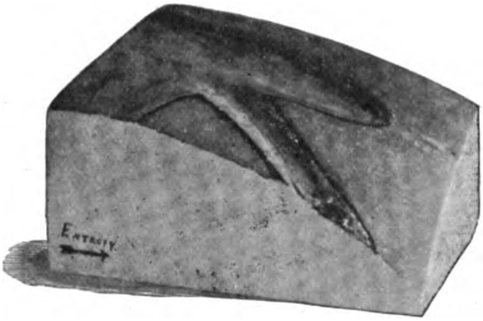


Fig. 4.

The choice of coördinates will then be, energy, vertically downward, in the three-dimensional model, volume, measured to the right, in the diagram, Fig. 1, and entropy vertically downward, in the same diagram. This choice is not inconsistent with the arrows in the upper left-hand corner of Fig. 1. The model which has been constructed in accordance with these considerations is shown in the accompanying figure.

It satisfies the general requirements as to slope and convexity. It represents the gaseous or vapor state as having in general the greatest volume and a great range of pressure, etc. One property, however, does not seem to be indicated by this model, nor do I see how to satisfactorily change it so that this can be done.

It has been deduced mathematically and shown experimentally that if a saturated vapor be expanded adiabatically, or isentropically, it may either become superheated or partially condense to liquid; in fact both phenomena can be shown with one substance, as for instance, chloroform above 127° C. becomes superheated, and below this temperature condenses, by sudden expansion, while at this temperature no visible effect is produced by either expansion or compression. That is, there is an isentropic which is at a particular point tangent to the "steam line," those on one side of it not touching it at all, while those on the other cut both in and out. This particular property is not shown by the model as constructed, with the present choice of coördinates. If, however, we had measured entropy horizontally in Fig. 1, then the isentropics, being vertical, might be tangent to, or cut through the steam line. This choice of coördinates has, however, seemed impossible for the reasons previously given.

We may, if we wish, discuss the question by a different method. The lines drawn in Figs. 1, 2 and 3 are all lines through the critical point. In Fig 2 the lines of constant pressure and temperature are tangent to the broken line; Fig. 3 shows the same property. In Fig. 2 the line of constant entropy cuts the broken line twice, but no other pair of lines has more than one intersection. Fig. 3 does not, as drawn, show the same property. In Fig. 2, passing from the water-line around the critical point in the homogeneous region to the steam-line, one cuts the lines in the following order: water-line, pressure, temperature, entropy, volume, pressure, temperature, entropy, steam-line. Fig. 3 gives the same order, with the choice of coördinates which we have adopted, if we let the temperature lines always slope downwards as due the pressure lines. With this change the two diagrams seem to agree, but otherwise their disagreement seems hopeless.

I shall be very glad to receive from anyone any suggestion which will help to remove the apparent disagreement between the two diagrams, or so modify the model that it may more completely represent the possible properties of actual bodies than it now seems to do.

THE UNIVERSITY OF CALIFORNIA, BERKELEY, CAL., Feb. 1, 1900.

ON THE TEMPERATURE OF THE ACETYLENE FLAME.[1]

By Edward L. Nichols.

CONCERNING the temperature of the acetylene flame, varying and incompatible statements are in existence. The temperature of combustion of this gas, according to Le Chatelier,[2] would be, when burned in air, from 2100° to 2420°. Measurements with Le Chatelier's pyrometer, on the other hand, made by V. B. Lewes,[3] gave temperatures lower than those of ordinary gas flames. Lewes found for the obscure zone 459°, for the edge of the luminous zone 1411°, and for the region near the summit of the luminous zone 1517°. Smithells,[4] upon the appearance of the data given by Lewes, described a series of experiments for the purpose of showing that the temperature of the flame is, in point of fact, very much higher than the values given by that author and that in many portions it is higher than the melting point of platinum.

It can be easily shown by inserting wires of that metal into the flat acetylene flame obtained from any one of the forms of burner usually employed, that while the thicker wires remain unmelted, those of very small diameter are readily fused. I found, for example, that a wire having a diameter of 0.0082 cm. became fused at the end with the formation of a distinct globule, before the metal had penetrated the outer luminous layer of the flame, whereas wires of 0.01 cm. or larger diameter remained unmelted. The experiments of Waggener, cited by St. John,[5] show that there are portions of the flame of the Bunsen burner in which it is possible to melt platinum, while MacCrae,[6] working with a platinum-rhodium element found for the hottest region in the Bunsen flame 1725°. It will be seen from the experiments to be described in this paper that MacCrae's determination, which was made with wires having a diameter of 0.02 cm. is not incompatible with the observations of Waggener and others. Smithells, in the paper just cited, describes the melting of platinum wires having a diameter of 0.01 cm. in various parts of the outer sheath of a flat flame of illuminating gas.

---

[1] A paper read at the meeting of the American Physical Society, February 24, 1900.

[2] Le Chatelier, Comptes Rendus, 121, p. 1144 (1895).

[3] Lewes, Chem. News, 71, p. 181 (1895).

[4] Smithells, Journal of the Chemical Society, 67, p. 1050 (1895).

[5] St. John, Wiedemann's Annalen, 56, p. 434 (1895).

[6] MacCrae, Wiedemann's Annalen, 55, p. 97.

Pellissier[1] in commenting upon Lewes's measurements refers to experiments in which minute wires of platinum, made by Wollaston's method of silver plating, drawing and subsequent dissolving of the silver coating, when thrust into the flame of a candle, melted instantly. I have not been able to find other printed reference to these observations and do not know with whom they originated. An attempt to repeat the experiment with a Wollaston wire resulted in the ready fusion of the wire by the flame. An examination of the remaining portions under the microscope showed that the metal had been melted down into clean well-rounded beads, and had not been consumed by oxidation or any other chemical reaction.

It will appear from the experiments to be described in this paper that Smithell's contention that the temperature of flames cannot be obtained directly from the indications of a thermo-element, for the reason that such an element loses heat by conduction and by dispersion from its surfaces so that the portions submerged in the flame never arrive at the temperature of the surrounding gases, is well founded. Lewes recognized this fact, and in his measurements made use of wires of three different sizes.

The Apparatus.

It is obvious that the indications of a thermo-couple which is submerged in a flame must depend upon the diameter of the wire, and it is upon this fact that my method for the estimation of the temperature of the acetylene flame is based. The flame selected for these measurements was of the usual flat type, obtained by the impinging of two jets of acetylene properly mixed with air, which issued from the orifices of one of the well-known forms of acetylene gas burner. The burner was mounted on a stand sliding upon a horizontal steel bar, so that a free lateral motion could be given to the flame. This stand moved so smoothly upon its metal support that the flame could be shifted by the direct action of the fingers of the operator with the same precision and freedom that we find in the well-adjusted draw tube of a microscope; and lateral displacements of about 0.01 cm., or even less could be made with ease. In some of the experiments requiring more accurate control, the flame was moved by means of a fine micrometer screw clamped to the bar. Upon a similar stand, attached to the same bar, was mounted one of the series of thermo-elements by means of which the determinations in question were to be made. The wires were all drawn from the same samples of pure platinum and platinum-rhodium (10%), obtained from Heraeus in Hanau, from stock believed to be identical with that tested by Holborn and Wien.[1] That thermo-elements made of wires differing in diameter, but all from the same stock, give with sufficient accuracy identical results, has been abundantly established. The hot junctions were given the form shown in Fig. 1. The platinum and platinum-rhodium wires were fastened by sealing into the fused ends of an open glass rod about 10 cm. in length, and 1 cm. in diameter, and the free ends of these wires were cut off, leaving about 3 cm. extending beyond the glass. These ends were pinched together in the form of a V, and while in contact were fused by a brief immersion in the oxy-hydrogen flame. The junction thus formed was trimmed off at right angles to the axis of the tube, until the thickness of the fused portion joining the two wires was as small as could be obtained without danger of rupturing the junction. It was found that by this process of trimming away with a pair of pocket scissors, better results could be obtained than by the use of file, or emery

---

[1] Pellissier; l'Eclairage a l'acétylène (Paris, 1897), p. 186.

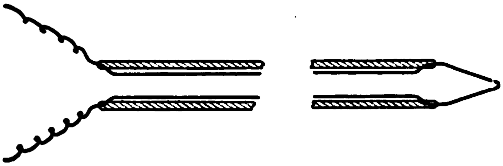


Fig. 1.

---

[1] Wiedemann's Annalen, 47, 107, and 56, 360.

paper, and that the preparation of junctions in this way was very easy and expeditious. It was almost always possible, after a few trials, to secure a junction in which the thickness of the connecting link of fused metal would not exceed 0.005 cm.

When viewed under the microscope, a junction thus prepared presents the appearance shown in Fig. 2. The surface left by trimming is smooth, and lies in a plane at right angles to the axis of the tube in which the wires of the thermo-element are mounted. When such a junction is brought near to the flat flame from an acetylene burner, its plane face lies parallel to the sheaths or layers of which the flame is composed and it is true, with a high degree of approximation, that the whole of the junction is in a surface of equal temperature.

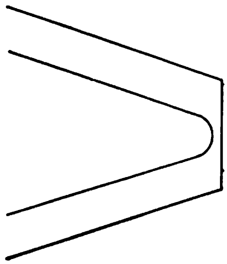


Fig. 2.

The electromotive force produced by the heating of the thermo-element in the flame was measured by means of a potentiometer consisting of a sensitive galvanometer of the D'Arsonval type and an accurately adjusted resistance box containing coils ranging from 50,000 ohms to 1 ohm. A large Clark cell of the old Feussner type was mounted in series with the resistance box. The thermo-element, the galvanometer, and a subsidiary resistance of 10,000 ohms were looped around a portion of the resistance box, the ratios being varied according to the Poggendorff method until complete balance was secured. The electrical connections are shown in Fig. 3. The type of standard cell selected for this work is subject to considerable errors from diffusion lag. It has, however, the advantage of being capable of furnishing a much larger amount of current than the small types of cell in which diffusion lag is avoided,

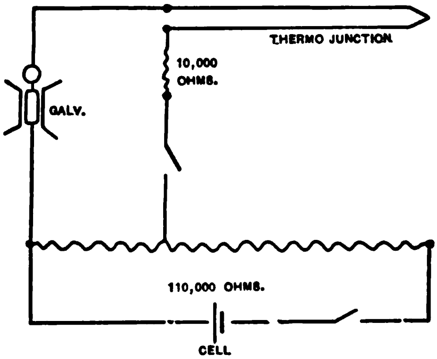


Fig. 3.

without appreciable loss of electromotive force. Two of these cells were placed side by side in a thick-walled inner room which had been constructed for the purpose of securing uniform temperature for the standard clock of the physical laboratory, and other similar apparatus. The range of temperature in this room fluctuated throughout the entire investigation between 18°C. and 19°C. The range was so small and the variations occurred so gradually that no changes of electromotive force of a size which it was necessary to consider in these measurements could have risen, other than those included in the usual correction for temperature.

The two cells were compared with each other from time to time by setting them in opposition to one another in circuit with a sensitive galvanometer and noting the deflection produced. It was found that although one of them was supplying current to the 100,000-ohm circuit of the potentiometer, during the times when it was necessary to close the key of that circuit, the difference of electromotive force between the used and unused cell was always very small, never more than a few hundred thousandths of a volt. At the end of the entire set of measurements, the difference was 0.00006 volt. The absolute electromotive force of these cells was checked by comparison with Clark cells of the H form and of the test-tube form, constructed in this department in 1898. As a result of these comparisons, it was found that the electromotive force of the cell used in the potentiometer might be taken at 1.430 volts at 18°.

For the convenient observation of the acetylene flame and of the distance from the median plane of that flame to the junction of the thermo-element, a long micro-camera was employed, the distance from the lens of which to the ground glass plate was about two meters. By means of a projecting lens of long focus, which was substituted for the ordinary photographic lens usually employed in this camera, an image of the flame and thermo-junction, enlarged to about seven diameters, was projected upon the ground glass. The plane of the flame was parallel to the axis of the camera so that those portions of the image of the former that were in focus were seen as if in cross section.

The stream of acetylene and air issuing at right angles to each other from the openings in the lava jet when viewed in the image, presented the appearance of cylinders or tubes with luminous walls, which under ordinary pressures united after meeting into a single stream of gas, the walls of which were very sharply defined and bright. Outside of this, and swaying back and forth with the drafts of air to which the flame is subjected, was an ill-defined image of the wings of the flame which consist of portions of the gaseous envelope which have lost their velocity and in consequence, the remarkable ability to resist deformation, possessed by the body of the flame. The thickness of the stable portion of the flame, which is surrounded by a well-defined luminous layer was found to be about 0.065 cm. The walls of brilliantly incandescent material around this central body were not more than 0.005 cm. in thickness. The appearance of these essential portions of the image of the flame on the ground glass is shown in Fig. 4, which is an outline sketch from a photograph of the flame. It will be noted that the body of the flame is not vertical, which arises from the fact that one of the jets is slightly stronger than the other. On account of the smallness of the openings in an acetylene burner, it is rare to find complete symmetry in these jets. The flame is almost always slightly thrown to one side as shown in the drawing, one face being convex, the other concave. These slight defects from symmetry do not in any way interfere with the measurement of the flame temperature.

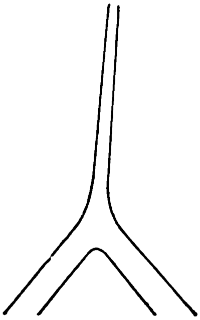


Fig. 4.

MEASUREMENTS OF THE ACETYLENE FLAME.

The wires selected for the four junctions to be used in the experiment upon the acetylene flame were measured under a microscope with micrometer stage. Their diameters were as follows:

TABLE I.

Junction	I.	Diameter	0.01996 cm.
"	II.	"	0.01598 "
"	III.	"	0.01089 "
"	IV.	"	0.00821 "

Readings were first made with junction I (diameter 0.01996 cm.). The flame was set at a distance of 6 mm. from the face of the junction and the potentiometer was balanced. The flame was then moved stepwise nearer and nearer, and the potentiometer rebalanced at each step until the face of the junction coincided with the edge of the luminous mantle at a point just above the apex of the inner non-luminous zone. (See *x*, Fig. 5.)

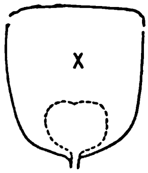


Fig. 5.

The rise of temperature indicated by the potentiometer readings is shown in curve *a* (Fig. 6), the data for which as well as for the other curves in that figure are contained in Table II. The increase of temperature is gradual at first, but at a distance of about 0.4 cm. from the center of the flame, the curve suddenly becomes steep. It is probable that this distance measures the thickness of the layer of non-luminous gas which surrounds the visible flame. Outside of this region the junction is heated almost altogether by radiation. As soon as it penetrates the column of moving gas, however, heat is brought to it principally by convection. Before the surface of the luminous mantle is reached the curve shows indications of approaching a maximum.

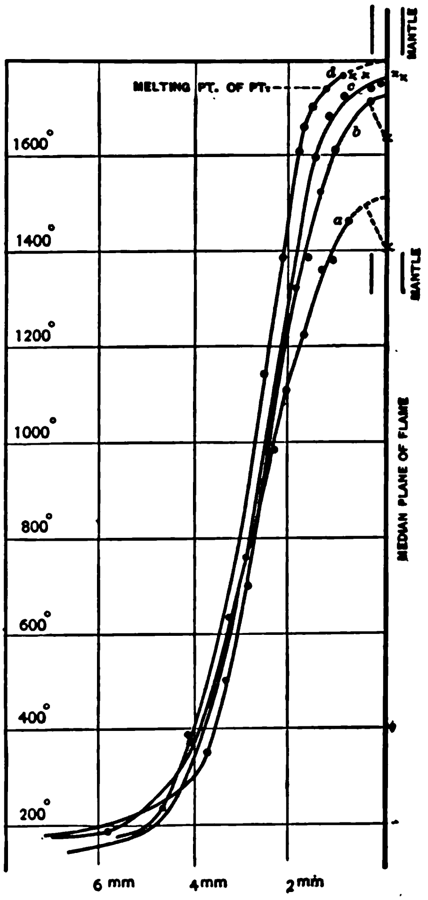


Fig. 6.

Upon pushing the flame still nearer to the junction, so that the latter penetrated the luminous region, an accumulation of lampblack began to form upon the wire, with fall of temperature. This process in the case of acetylene is so rapid that at the end of two minutes a button of carbon several millimeters in diameter is formed.

Table II.

Temperatures indicated by thermo-junctions I, II, III and IV, at various distances from the median plane of the acetylene flame.

Junction I.		Junction II.		Junction III.		Junction IV.	
Distance.	**Temp.**	**Distance.**	**Temp.**	**Distance.**	**Temp.**	**Distance.**	**Temp.**
mm.		mm.		mm.		mm.	
5.62	185°	——	——	5.42	165°	4.63	233°
3.91	370°	3.65	353°	4.82	183°	4.11	406°
2.85	760°	3.33	508°	3.21	657°	2.55	1168°
2.09	1128°	2.90	695°	2.03	1278°	2.12	1411°
1.66	1229°	2.30	989°	1.50	1598°	1.86	1613°
1.30	1367°	1.93	1322°	1.18	1685°	1.70	1667°
1.07	1382°	1.68	1385°	0.894	1724°	1.54	1705°
0.850	1467°	1.40	1513°	0.566	1747°	1.30	1738°
		1.09	1617°	0.238	1759°	1.025	1771°
		0.320	1715°	0.00	1775°	0.780	molten
				−0.29	molten	0.300	molten

This is finally torn loose from the wire by its own weight; whereupon the deposition of a new mass begins. I attempted, by watching the breaking away of the carbon from the wire, which occurred at regular intervals, to determine the temperature of the wire before the coating of carbon had begun to show itself again. The highest temperature which it was possible to observe in this way was nearly one hundred degrees below that in the luminous layer, and it was obvious from the movement of the galvanometer needle that the junction was being rapidly cooled.

Junction II. (diameter 0.01598 cm.) was now substituted for junction I., and a similar set of readings were made. This junction, as had been anticipated, showed higher temperatures. It was found possible, owing to the small diameter and consequently high temperature of the wire, to penetrate further into the flame before the deposition of carbon began; so that measurements with the junction actually within the luminous layer could be made. The general form of the curve, as will be seen by inspection of the figure (curve *b*) is the same as that obtained with junction I. After penetrating the luminous mantle to a small fraction of a millimeter, carbon began to gather upon this junction likewise, with lowering of temperature as in the case of junction I. The attempt to read temperatures immediately after the dropping of the accumulated carbon showed that the highest temperature which could thus be observed was again about one hundred degrees below the temperature of the luminous mantle. It was clear in this case, as before, from the rapid fall of temperature already going on, that this reading has no significance.

Similar readings with junction III. (diameter 0.0108 cm.) gave a third curve of the same type as those plotted from the readings made with I. and II.; but the temperatures were higher throughout. With this junction, it was found possible to penetrate to the center of the flame without the deposition of carbon, the temperature of the wire being apparently too high to permit the formation of soot. Upon pushing through the median plane of the flame to the second luminous mantle the junction was melted. This result was not unexpected, since the temperature of the junction at the first luminous mantle reached 1750°, so that a rise of twenty-five degrees of temperature would suffice to produce fusion. The wire when pushed through the flame in the manner just described is heated for greater and greater distances back from the junction until the losses of heat at the junction are sufficiently diminished to raise the tips of the wires to the melting-point.

With junction IV. (diameter 0.0082 cm.) a fourth curve, similar in form to the preceding ones and with still higher temperatures was obtained. This junction was fused at a distance of 0.075 cm. from the core of the flame, and of 0.037 cm. from the edge of the first luminous mantle. It was easy to observe in the enlarged image upon the plate of the micro-camera, the melting away of the platinum wire, while the platinum-rhodium alloy was still unaffected, and while contact was still unbroken. A satisfactory observation of the electromotive force of the thermo-element at the melting-point of platinum was thus obtained. This reading (0.018236 volt) differs from the value found in my calibration of the thermo-junctions used in this investigation (0.018262 volt) by a quantity (0.000026 volt) less than the errors due to changes in the electromotive force of the standard cell. If the latter reading be taken to correspond to 1775° the former indicates 1773°.

Beyond this point it was impossible to make direct observations of temperature; but the form of this and the preceding curves were so closely allied that I felt no hesitation in extending the curve *d* to the core of the flame. This has been done by means of dotted lines in the figure. Curves *a* and *b* have been extended in the same manner. In order to form an estimate of the temperature which would have been reached by a thermo-junction of negligible cross-section, provided such a junction could have been obtained which was capable of registering temperatures above that of the melting point of platinum, the ordinates of the four curves *a*, *b*, *c* and *d* were taken for the core of the flame, for the plane of the luminous mantle, for a plane distant 0.07 cm. from the core, and for a plane 0.10 cm. from the core. These readings were plotted and curves were drawn through them as shown in Fig. 7; relative cross-sections of the wires being taken as abscissæ, the temperatures as ordinates. If these curves could be extended to the line representing zero cross-section, the temperatures indicated by the points in which each of them cuts that line would give the temperature of the portion of the flame to which the curve corresponds. There is a considerable element of uncertainty in extrapolation even

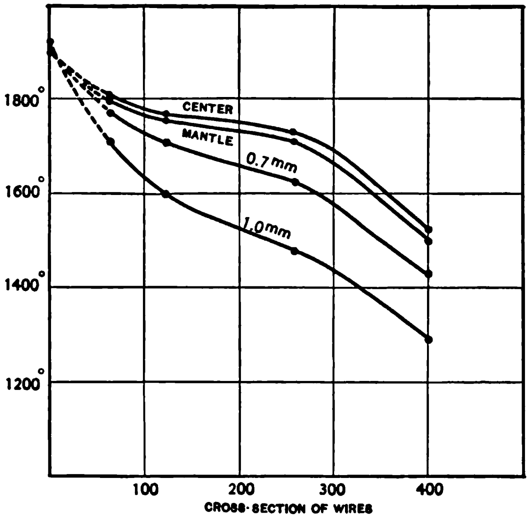


Fig. 7.

over so short a range as this; but it is obvious from the character of the curves lying within the limits of observation that each of them trends upward and it seems highly probable that they all meet the line of zero cross-section at a temperature not far from 1900°. The fact that the curves cut this line at nearly the same temperature would seem to indicate that the distribution of temperatures from the center of the flame outward for a distance of about 1 mm. is a nearly uniform one.

It would, perhaps, be unwise to attempt to draw any more definite conclusion from the probable trend of these curves; but I have ventured to extend them in the manner shown in the figure, so that the curve for the region 1 mm. from the center of the flame reaches the zero of abscissæ about twenty degrees above that for the center of the flame, *i. e.*, at 1920° and the intermediate curves at temperatures lying between. I regard this as an extreme treatment of the case, and allude to it only to indicate that, in accordance with common belief, the highest temperature may be found in the outer non-luminous layer of the flame; but that it is unlikely that the difference amounts to more than twenty degrees.

The point of intersection referred to above lies more than one hundred degrees above the highest temperature recorded by even the smallest of the thermo-elements, and it is safe to infer that nearly all previous attempts at the measurement of flame temperatures must, for lack of correction of the error, due to loss of heat through the wire, be regarded as much too low. The Junction IV. is, so far as I am aware, the smallest in cross section that has been used in such work. With larger wires, the correction for loss of heat would be even greater, except in cases where, as in the observations made by Smithells, pains were taken to immerse an extended portion of the wires within the flame.

TEMPERATURE OF OTHER FLAMES.

For the purpose of comparison, I measured in a manner analagous to that just described, the temperature of the luminous flame of ordinary illuminating gas, and the flame of a candle. The gas flame employed for this purpose was obtained from a lava tip, rated at one cubic foot and giving a flat flame of the usual form. The image of this flame, when viewed upon the ground-glass screen of my camera was found to be comparatively ill-defined and unsteady; but although the outlines of the luminous sheath were much less clearly marked than in the case of the acetylene flame, they were discernible. Owing to the continued motion, due to the small velocity of the gas issuing from the jet, no attempts were made to plot curves of temperatures outside the flame. All readings were made with the junction as nearly as possible in contact with the outer surface of the luminous sheath, at a point in the brightest portion of the flame. This position is approximately in dicated by the letter *x* in Fig. 8. The four junctions already described were mounted, one after another, in such a position that the flame could be moved up until they came into contact with the sheath at the point indicated. The temperatures of the junctions when in that positiona are given in the following table:

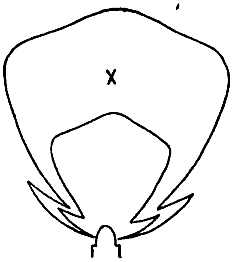


Fig. 8.

TABLE III.

Junction I.	1385°
Junction II.	1484°
Junction III.	1609°
Junction IV.	1676°

These values having been plotted with relative cross sections of the wires as abscissæ, and temperatures as ordinates were found to lie upon a smooth curve (*g*) as shown in Fig. 9. This curve when extended to the line corresponding to zero cross section gave for the temperature of the flame 1780°, a temperature sufficient to account for the success of Smithells's experiment, already described, in which platinum wires of small diameter were melted in the outer sheath of such a flame. I found it easy, by holding a wire of the size used in junction IV. in a plane parallel to that of the flame, and moving it gradually toward the latter, to verify his statement. The wire was readily melted.

It was not thought necessary to make further experiments upon this flame. The region selected was, so far as one could judge from the brightness of the luminous sheath, the hottest portion of the flame. My measurements upon this region would lead to the conclusion that *the luminous sheath of ordinary gas flames is at least one hundred and twenty degrees lower than the corresponding region in the acetylene flame.* Luminous flames of ordinary illuminating gas would perhaps repay further study, but owing to the fact that such gas is an ever-varying mixture and that it is burned under conditions of pressure, etc., such as to give a fluctuating character to the flame, the problem would have at best an indefinite character from which studies of the acetylene flame are free. In the latter case we have to deal with a definite fuel and the velocity of the jets of gas from the burner is sufficient to give a high degree of stability.

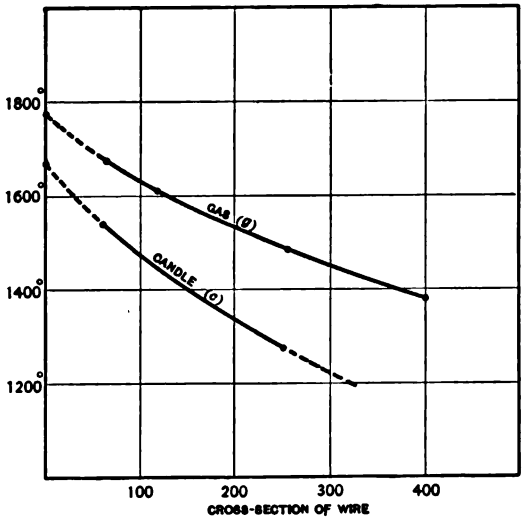


Fig. 9.

The candle would seem an even less satisfactory subject of study in these respects than illuminating gas, but the fact of the melting down of Wollaston wire in a candle flame, the verification of which I have briefly described in the opening paragraphs of this paper, seemed to discredit so completely the lower values commonly assigned to the temperature of such flames that I decided upon a redetermination.

The fact that the flame of a candle, mounted upon a fixed stand, would move steadily downward as the material of which it was composed burned away, made it convenient without any serious modifications of my apparatus, to explore the temperature of the luminous sheath throughout the entire length of the flame. It was only necessary for this purpose to mount a candle upon the steel bar in the position previously occupied by the acetylene flame and when it had reached such a length that the level of the rim of the cup lay below the level of the junction, to move the candle toward the latter by means of a micrometer screw until the junction began to be submerged in the luminous sheath of the flame. It was then easy by a series of slight adjustments of the flame, to explore with the junction the entire surface of the luminous sheath from base to tip, measuring temperatures from time to time, and determining the position by means of the height of the junction above the rim of candle cup. The latter observations were readily made by means of the image of the candle upon the ground glass of the camera. Explorations of the candle flame in the manner described were made with junctions II. and IV. and the results obtained showed a degree of consistency much greater than the fluctuating character of the source under observation had led me to expect. Both sets of observations showed a maximum of temperature in the same region; that lying just above the tip of the interior dark zone of the flame. Readings were made by watching the movements of the candle flame and securing a balance of the potentiometer at times when the face of the junction was as nearly as possible in contact with, but not deeply submerged within the luminous layer. Whenever the wire plunged to any considerable depth beyond the luminous surface, deposition of soot occurred with lowering temperature, and it was necessary to withdraw it into the non-luminous regions outside and to wait until the deposit had been burned off, before proceeding with the readings. In computing the actual temperatures of the luminous sheath of the flame from these readings, I contented myself with the following rough approximation. The maximum temperatures shown by junctions II. and IV. were plotted upon the same diagram used for the luminous gas flame. These temperatures were 1281° and 1546°; values which, as will be seen by inspection of Fig. 9 (*c*), lie much below those of the corresponding readings for the luminous gas flame, but in such positions as to make it easily possible to draw through them a curve analagous in form to that obtained for the latter. Such a curve would cut the line of zero cross section at about 1670° which may, I believe, be taken as the approximate temperature of the hottest portions of the luminous sheath of the candle flame. Estimates of this temperature by the probably less accurate methods of drawing a straight line through the points in question and taking the points in which this line cuts the line of zero cross section to be the temperature of the flame, and estimates based upon the assumption that the true temperature is as many degrees above the temperature indicated by junction IV. for the candle as it is for the gas flame, would lead to values respectively twenty-four degrees and forty degrees lower than that obtained by the method which I have adopted. I believe that the temperature just given (1670°) is much closer to the truth than that obtained under either of the other assumptions. Estimated temperatures for other portions of the luminous sheath were made by assuming that the correction to be applied to the readings obtained with junction IV. would be the same in all positions. These values are given in Fig. 10 which may serve in place of an ordinary table. The portions of the flame to which each reading refers are more readily indicated by giving such a diagram of the flame than in any other way.

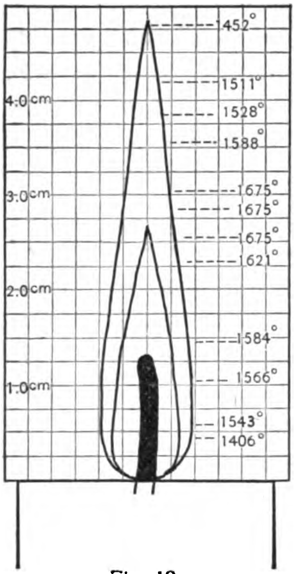


Fig. 10.

The fact that, in the case of the acetylene flame and the ordinary gas flame, this method gives values high enough to account for the melting of platinum, but leads to an estimate of the temperature of the candle flame which is about one hundred degrees below the melting point of that metal, would seem, at first sight, to throw the procedure into serious doubt. My experience with the method has, however, been such as to make an error of one hundred degrees in the estimation of the candle-flame temperature seem highly improbable. Messrs. Lummer and Pringsheim in a recent communication to the German Physical Society[1] give an estimate of the temperature of candle flames based upon a relation which they have established between the position of the maximum in the energy curve of the spectrum of a source of light and its temperature. Assuming the the radiating substance in the flame to have the properties of a *black body* they find this temperature in the case of the candle flame to be 1687°, a value but seventeen degrees above that which I have given.

To account for the fusion of Wollaston wire in the flame of a candle, one might consider the possibility of the existence in such a flame of layers of gas the temperature of which is much above the surrounding regions, and that these layers may be so thin that it would not be possible to submerge the thermo-junction completely in them. In such a case, the junction would give a value approximate to the average of the temperatures of the gases with which it was brought into contact. Before assuming this structure of the flame, which really has nothing to support it save the necessity of accounting for the apparent discrepancy which I have just pointed out, it seemed wise to consider, on the other hand, whether the melting point of the Wollaston wire was necessarily that of pure platinum. Such wires would be made of ordinary commercial metal, the melting-point of which might vary considerably from that of the purer platinum used in the determination of the temperature of fusion. It is likewise readily conceivable that in the process of drawing within the silver coating, a certain amount of silver might be worked into the pores of the platinum and not removed by the subsequent action of the nitric acid. The determination of the melting-point of even such minute wires is fortunately a simple matter by means of the form of thermo-element used in the experiments which I have just described. It is only necessary to wrap a piece of the wire to be tested around the junction as shown in Fig. 11, to cut it off so that the ends of the loop extend slightly (about 0.05 cm.) beyond the face of the junction; and having mounted the junction in the usual manner, to move the acetylene flame up to it by means of a micrometer screw, watching the enlarged image of junction and flame upon the ground glass of the micro-camera. The wire will then begin to fuse at its outer end, and the fusion, following upon the progressive movement of the flame, can be watched until the loop is melted back to the surface of the junction itself. With such precision can this progressive melting of the wire be carried out in a room free from draughts, that it is possible to melt away the loop of wire in front of the junction until no part of it protrudes beyond the smooth face of the latter, and yet leave that portion of the loop which extends behind the wire of the junction and is protected by it, unmelted. I performed this experiment with a piece of the same Wollaston wire which I had succeded in melting in the candle flame ; and I found its melting point, as indicated by the electromotive force of the junction, to be 1674°. To test the question whether this very low melting-point was due to the presence of silver undissolved by the nitric acid, a piece of the same wire was left in the acid for twelve hours, after which the melting-point was again tested in the manner just described. The result of this determination was 1687°. The latter reading was, I think, too high, since subsequent examination under the microscope showed that the loop of the wire behind the junction had been melted, so that the junction was probably a few degrees too hot. At any rate, it may safely be concluded from these determinations that the melting point of the Wollaston wire was at least one hundred degrees lower than that of pure platinum, and that the fusion of such wire in the candle flame did not necessarily indicate that my estimate of the temperature of the flame was at fault.

---

[1] Lummer and Pringsheim; Verhandlungen der Deutschen Physikalischen Gesellschaft; 1899, p. 214.

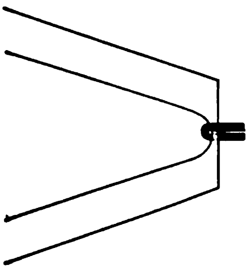


Fig. 11.

This point I deemed it worth while to confirm by direct observation of the melting-point of the Heraeus platinum ; for which purpose a piece of the wire used in junction IV. was rolled into a flat strip. From this strip a piece about 0.01 cm. broad was cut, which was looped around the junction in the manner just described. The melting-point was found to be 1775°. Experiments upon the fusion of gold, platinum, palladium and other metals made in this way, have convinced me abundantly of the delicacy and reliability of this means of determining melting-points. It is free from many of the most serious difficulties involved in the procedures usually employed. The details of this method, which is especially applicable to the calibration of thermo-elements, I propose to describe at some length under a separate title.

Scale of Temperatures.

The temperatures given in this paper are based upon a scale arbitrarily adopted, in which the melting-point of pure platinum is taken at 1775°, and gold at 1070°. Nothing more is claimed for this scale than that the reference points are those most easily reproducible in high temperature work, so that any one having occasion to use the data given can make reductions to whatever scale of temperatures he desires to employ. Up to the present time, high temperature work has not reached a stage in which it is possible to do more than to refer readings to these melting-points. The assignment of the proper degrees centigrade to the melting-points of gold and platinum appears as yet to be a somewhat uncertain matter, but the behavior of platinum—platinum-rhodium thermo-elements appears to be in so far consistent, that when the temperatures of these two points are fixed, but little doubt need be felt concerning the relative values of the intervening temperatures. Various wires even from the same maker will give electromotive forces that differ slightly one from another, but in general it may be said that with pure platinum—platinum-rhodium (10%) elements from a given stock of metal, the character of the curve will always be closely the same. Since my materials were from the same source as those used by Holborn and Wien, I have contented myself with the use of their curve, merely shifting the ordinates so as to make it pass through the points corresponding to the electromotive forces at the melting-points of gold and platinum as determined by myself. Various calibrations made in the course of other work appear to justify this procedure. These calibrations I hope to discuss at considerable length in a forthcoming paper. Whatever changes it may in future be found necessary to make in the assignment of these fixed points can readily be extended to all my data, by any one who may care to do so.

To Mr. L. W. Hartman, who aided me in making many of the observations described in this paper, I desire to express my indebtedness.

Physical Laboratory of Cornell University, February 21, 1900.

NOTE ON THE LAW OF DISTRIBUTION OF VELOCITIES AMONG GAS MOLECULES.

By N. D. C. Hodges.

LET us consider one molecule. Let this molecule pass from a point A, to a point B, the time of passing being such that the molecule shall have come in contact with as many molecules as there are in the unit of volume. By taking a unit of volume large enough, the molecule considered will, when it reaches B, have passed through all possible velocities, and through each velocity in proportion to the frequency of that velocity among the molecules, *i. e.*, according to the law of distribution, if such exists.

The system consisting of the single molecule and all the other molecules is a conservative system. The sum of the kinetic energy of the single molecule and that of all the others, remains constant. The principle of least action applies.

Any particular velocity being represented by c, the number of times the molecule will have a velocity between c and $c + dc$ in passing from A to B, will depend on N, the total number of molecules in unit of volume, on some function of c, $\varphi(c)$, the form of which is to be determined, on dc, and on a constant A dependent on the temperature of the gas and the molecular weight.

The kinetic energy at any instant will be expressed by $ANc^2\varphi(c)dc$ and the action by $AN\int_0^\infty c^2\varphi(c)dc$.

That this may be a minimum, $\varphi(c)$ must have the form $\frac{\sqrt{h}}{\sqrt{\pi}}e^{-hc^2}$. Hence the expression for the action becomes

$$\frac{AN\sqrt{h}}{\sqrt{\pi}}\int_0^\infty c^2e^{-hc^2}dc.$$

As the variation in the action due to changes of temperature is involvde in A, this expression is solely dependent on the number of velocities the molecule passes through, *i. e.*, it equals N. $\therefore A = 4h$. And the law of distribution is

$$\frac{4Nh^{\frac{3}{2}}}{\sqrt{\pi}}e^{-hc^2}c^2dc.$$

Harvard University, Cambridge, Mass.

NEW BOOKS.

The Elements of Physics. By HENRY CREW, PH. D. Pp. xiv + 347. The Macmillan Co., New York, 1899.

It is not often that the reading of an elementary text-book of physics can be referred to as distinctly refreshing, and yet in the case under consideration this is exactly true. The author writes in so lively a style, with so much enthusiasm for the subject, and with so appreciative a recognition of the value of the experience that the average boy has at his disposal in beginning the study of physics, that the attention is arrested at the outset.

The usual order of development of the subject is followed, but the treatment is by no means commonplace. In the discussion of a topic the author usually follows the order of the historical development of the subject, as being in most cases the natural approach. There is a notable exception to this, however, in the treatment of mechanics, where the usual method is followed, of beginning with motion and later taking up cases of equilibrium. It seems to us that in introducing *beginners* to so difficult a subject as mechanics, we would do well to follow the guidance of history and begin with questions of equilibrium before taking up problems involving acceleration with their attendant difficulties. Of course the method adopted has advantages when the object in view is the systematic presentation of the subject, and this is, doubtless, why it is so generally employed in spite of its disadvantages from a pedagogical standpoint.

The excellent plan has been adopted of giving the names of distinguished physicists in connection with their discoveries, and with each in brackets the dates of birth and death; thus almost unconsciously the student comes to have a historical perspective of the subject. The author's interest in the history of the subject leads him to reprint the very interesting original account of Boyle's experiments on the "Spring of the Air," and in the case of Torricelli's experiment to come to the defense of the old *horror vacui* theory in a way that is quite amusing.

A treatment of water waves that is unusually complete for an elementary text-book is an excellent feature. The change of front due to retardation in shoal water is discussed and used to advantage later in considering the refraction of light waves. It seems to us that in most of our text-books the subject of surface waves receives very scant treatment, considering its importance.

In treating the subject of electrical resistance the author prefers to approach it from the heating effect of the current, and does not take up Ohm's law till almost the last topic in his treatment of currents, though it would seem to us that the early introduction of the point of view of electric currents expressed by Ohm's law might be a help to the student.

Special references to other books are given at the ends of some of the chapters, with comments as to the character of the treatments referred to. These will doubtless be very helpful to a student wishing to go more extensively into some part of the subject.

The whole treatment, though elementary, is exact and quantitative, and numerous problems are given. The student who masters this textbook will have to *think*, but he will find himself stimulated and helped in his thinking by a natural presentation of the subject, by clear statements, lucid explanations and apt illustrations.

The general arrangement is clear and systematic, good use being made of different styles of type in emphasizing important points.

On the whole the book is to be distinctly approved as a freshly-written modern presentation of the subject, elementary, but exact and thorough, and well adapted for advanced high school instruction. The teacher will find in it many valuable suggestions. The author is a teacher of experience, who has evidently given thought to the art of presenting the subject so that it may be most readily grasped by the pupil.

ARTHUR L. KIMBALL.

A Text-book of Physics. By W. WATSON. London, Longmans, Green & Co., 1899. Pp. xxii + 896.

This addition to the rapidly growing list of physics text-books contains many notably commendable features. One recognizes in it at once the work of a skillful writer, an experienced teacher and a working physicist. Mr. Watson's volume is comprehensive in the sense that it states nearly every essential principle of physics, says something on all of the important phenomena, and describes a great number of the methods by which our knowledge of the leading facts of the science has been attained. There is likewise a wealth of well selected tabular data interspersed throughout the book. The volume is bulky, consisting of nearly 900 pages of closely printed matter, but when we turn to any given topic we find that its bulkiness is not due to the introduction of any considerable amount of detail in the description or in the theoretical discussions, but is simply a symbol of the enormous amount of material which has to be included in any treatise covering the domain of modern physics. The individual topics indeed are very briefly considered, and one is inclined to quarrel with them on the score of omission rather than because they are overloaded with unnecessary matter. The author's statements are for the most part clear, and in accordance with the accepted views of physicists.

One finds, as he must expect to find in any such treatise, minor points concerning which he would take issue; as, for example, where electromotive force and difference of potential are specifically stated to be different names for one and the same thing, or where, as in the diagram on page 516, wave-lengths of light are expressed in centimeters multipled by 10^{-10} (the minus sign in the exponent is omitted; a misprint that is not likely to mislead any one), instead of using, as elsewhere in the volume, one of the two accepted systems. It is true that the reference of wave-lengths to a unit thus simply related to the centimeter might well be preferred to either of those which have come into use, but it is a question whether in a text-book it is well to depart from the general practice, especially as the author in subsequent pages reverts to the beaten path.

These, however, are matters of little moment. The important thing is that Mr. Watson has produced for us a treatise on experimental physics, which contains brief and accurate statements of so many of the important topics in physics. The value of the book as a reference work would have been greatly increased by the introduction of citations of original sources, and this remark applies particularly to the well-selected tables of physical constants. Many of the illustrations are new, and some of them will be welcome additions to the store of illustrative material already at the command of the teacher.

We are glad to note what is really very unusual among British writers, even of the modern school: that is, a fairly consistent adherence to the gram, the centimeter and the second as fundamental units.

E. L. N.

Standard Text-Books on General Physics

Elements of Theoretical Physics

By DR. C. CHRISTIANSEN

Professor of Physics, University of Copenhagen

TRANSLATED INTO ENGLISH BY

W. F. MAGIE, Ph.D., Professor of Physics, Princeton University

Cloth. 8vo. $3.25, net

Problems and Questions in Physics

CHARLES P. MATTHEWS, M.E., AND JOHN SHEARER, B.S.

Purdue University *Cornell University*

8vo. Cloth pp. 247 + 4. Price $1.60, net

An Intermediate Course of Practical Physics

By A. SCHUSTER, Ph.D., F.R.S., AND C. H. LEES, D.Sc.

Owens College, Manchester *Owens College, Manchester*

12mo. Cloth. pp. xv + 248. Price $1.10, net

Lessons on Elementary Practical Physics

By BALFOUR STEWART, A.M., LL.D., F.R.S., AND W. W. HALDANE GEE

Vol. I. **General Physical Processes.** 12mo. $1.50, *net*
Vol. II. **Electricity and Magnetism.** $2.25, *net*
Vol. III. Part I. **Practical Acoustics.** $1.10, *net*
Part II. **Heat and Light.** *In Press*

Laboratory Manual of Physics and Applied Electricity

Arranged and Edited by EDWARD L. NICHOLS

Professor of Physics in Cornell University

IN TWO VOLUMES

Vol. I. Cloth. Price $3.00, net

JUNIOR COURSE IN GENERAL PHYSICS. By ERNEST MERRITT and FREDERICK J. ROGERS.

Vol. II. Cloth. pp. 444. Price $3.25, net

SENIOR COURSES AND OUTLINES OF ADVANCED WORK By GEORGE S. MOLER, FREDERICK BEDELL, HOMER S. HOTCHKISS, CHARLES P. MATTHEWS, and THE EDITOR.

A large proportion of the students for whom primarily this Manual is intended are preparing to become engineers, and special attention has been devoted to the needs of that class of readers. In Vol. II., specially, a considerable amount of work in applied electricity, in photometry, and in heat has been introduced.

THE MACMILLAN COMPANY

New York: 66 Fifth Avenue

BOSTON CHICAGO SAN FRANCISCO

A History of Physics

In Its Elementary Branches, including the Evolution of Physical Laboratories

By FLORIAN CAJORI, Ph.D.

Professor of Physics in Colorado College.

Cloth Cr 8vo, $1.60 *net.*

This brief popular History gives in broad outline the development of the science of physics from antiquity to the present time. It contains also a more complete statemen. than is found elsewhere of the evolution of physical laboratories in Europe and Americat The book, while of interest to the general reader, is primarily intended for students and teachers of physics. The conviction is growing that, by a judicious introduction of historical matter, a science can be made more attractive. Moreover, the general view of the development of the human intellect which the history of a science affords is in itself stimulating and liberalizing.

"Prof. Cajori has followed his 'History of Mathematics' with a 'History of Physics' that is *even more interesting and valuable.* . . . Every teacher and student of physics should read it and add it to his library. It shows careful study, enthusiasm and a comprehensive grasp of the subject."—*Journal of Education*

"A most valuable contribution to the subject."—*Scientific American.*

"Probably this will prove the most successful of all Prof. Cajori's histories."
—*The Nation.*

BY THE SAME AUTHOR

A History of Mathematics

Cloth 8vo, $3.50 *net.*

"What we have a right to expect in such a handbook is an agreeable narrative of the most material events in the history of mathematics, and this Professor Cajori incontestably supplies. The book was much wanted."—*The Nation.*

"A scholarship both wide and deep is manifest in this *History of Mathematics* which the author has infused with his own ardor in this department of science."
—*Journal of Education.*

"To the student with a love for mathematical science this book will be as entertaining as a romance."
—*The Transcript*, Boston.

A History of Elementary Mathematics

Cloth 12mo, $1.50 *net.*

"A most instructive, and at the same time a very readable piece of work, full of curious facts."—*The Bookman.*

"By no means an abridged edition of *The History of Mathematics.* It is an entirely new book, giving a somewhat detailed account of the rise and progress of Arithmetic, Algebra and Geometry. The book should be read by all teachers of these subjects, and by mathematical students generally."
—*American Mathematical Monthly.*

"The product of wide and scholarly research . . . For its historical facts and its suggestiveness, this work should be read carefully by all students and teachers of mathematics."—*The Dial*, Chicago.

THE MACMILLAN COMPANY, Publishers, New York

THE CHEAPEST BOOKS IN

Science, Philosophy and Psychology

Now Publishing in America

High Grade Paper. **Large Print.** **Stitched Backs**

THE RELIGION OF SCIENCE LIBRARY

No. 1. **The Religion of Science.** By Paul Carus. 25 cents (1s. 6d.).
2. **Three Introductory Lectures on the Science of Thought.** By F. Max Müller. 25 cents (1s. 6d.).
3. **Three Lectures on the Science of Language.** By F Max Müller. 25 cents (1s. 6d.).
4. **The Diseases of Personality.** By Th. Ribot. 25 cents (1s. 6d.).
5. **The Psychology of Attention.** By Th. Ribot. 25 cents (1s. 6d.).
6. **The Psychic Life of Micro-Organisms.** By A. Binet. 25 cts. (1s. 6d.).
7. **The Nature of the State.** By Paul Carus. 15 cents (9d.).
8. **On Double Consciousness.** By Alfred Binet. 15 cents (9d.).
9. **Fundamental Problems.** By Paul Carus. Pp., 373. 50 cents (2s. 6d.).
10. **The Diseases of the Will.** By Th. Ribot. 25 cents (1s. 6d.).
11. **The Origin of Language** and **The Logos Theory.** By Ludwig Noiré. 15 cents (9d.).
12. **The Free Trade Struggle in England.** By Gen. M. M. Trumbull. 25 cents (1s. 6d.).
13. **Wheelbarrow on the Labor Question.** 35 cents (2s.).
14. **The Gospel of Buddha.** By Paul Carus. 35 cents (2s.).
15. **Primer of Philosophy.** By Paul Carus. 25 cents (1s. 6d.).
16. **On Memory,** and **The Specific Energies of the Nervous System.** By Prof. Ewald Hering. 15 cents (9d.).
17. **The Redemption of the Brahman.** By R. Garbe. 25 cents (1s. 6d.).
18. **An Examination of Weismannism.** G. J. Romanes. 35 cents (2s.).
19. **On Germinal Selection.** By August Weismann. 25 cents (1s. 6d.).
20. (Out of print.)
21. **Popular Scientific Lectures.** By Ernst Mach. 50 cents (2s. 6d.).
22. **Ancient India. Its Language and Religions.** By Prof. H. Oldenberg. 25 cents (1s. 6d.).
23. **The Prophets of Israel.** By Prof. C. H. Cornill. 25 cents (1s. 6d.).
24. **Homilies of Science.** By Dr. Paul Carus. 35 cents (2s.).
25. **Thoughts on Religion.** By G. J. Romanes. 50 cents.
26. **The Philosophy of Ancient India.** By R. Garbe. 25 cents (1s. 6d.).
27. **Martin Luther.** By Gustav Freytag. 25 cents (1s. 6d.).
28. **English Secularism.** George Jacob Holyoake. 25 cents.
29. **On Orthogenesis.** By Th. Eimer. 25 cents (1s. 6d.).
30. **Chinese Philosophy.** By Paul Carus. 25 cents (1s. 6d.).
31. **The Lost Manuscript.** By Gustav Freytag. 60 cents (3s.).
32. **A Mechanico-Physiological Theory of Organic Evolution.** Carl von Nägeli. 15 cents (9d.).
33. **Chinese Fiction.** By Dr. George T. Candlin. 15 cents (9d.).
34. **Mathematical Essays and Recreations.** By H. Schubert. 25 cents (1s. 6d.).
35. **The Ethical Problem.** By Paul Carus. 50 cents (2s. 6d.).
36. **Buddhism and its Christian Critics.** Paul Carus. 50 cents (2s. 6d.).
37. **Psychology for Beginners.** Hiram M. Stanley. 20 cents (1s.).
38. **Discourse on Method.** Rene Descartes. 25 cents (1s. 6d.).
39. **The Dawn of a New Era.** Paul Carus. 15 cents (9d.).
40. **Kant and Spencer.** Paul Carus. 15 cents (9d.).

THE OPEN COURT PUBLISHING COMPANY
324 Dearborn Street, Chicago, Ill.
London: Kegan Paul, Trench, Trübner & Co., Ltd.

The Astrophysical Journal

AN INTERNATIONAL REVIEW OF SPECTROSCOPY AND ASTRONOMICAL PHYSICS

EDITED BY
GEORGE E. HALE
Director of the Yerkes Observatory
AND
JAMES E. KEELER
Director of the Lick Observatory
WITH THE COÖPERATION OF
THE LEADING ASTROPHYSICISTS OF THE UNITED STATES AND EUROPE

Contents of Recent Numbers

OCTOBER 1899.

The Effect of Certain Impurities on the Spectra of Some Gases Percival Lewis
On Professor Keeler's Photometric Researches, by Photographic Methods, on the Nebula in Orion . . . J. Scheiner
Note on the Foregoing Article by Professor Scheiner . . . . James E. Keeler
The Position of the Stars of Type IV and of the Variable Stars of Type III in Reference to the Milky Way . T. E. Espin.
Observations of Comet Spectra W. H. Wright.
The Spectroscopic Binary Capella . W. W. Campbell
The Variable Velocities in the Line of Sight of ε Librae, h Draconis, λ Andromedae, ε Ursae Minoris and ω Draconis W. W. Campbell.
The Variable Velocity of α Ursae Minoris in the Line of Sight W. W. Campbell
The Variable Velocity of Polaris Edwin B. Frost
The Wave-Length of the Green Coronal Line, and Other Data Resulting from an Attempt to Determine the Law of Rotation of the Solar Corona W. W. Campbell
The Ring Nebula in Lyra James E. Keeler
Pressure in the Electric Spark John Fred Mohler
Minor Contributions and Notes

NOVEMBER 1899.

On the Relative Brightness of the Planets Mars and Jupiter, from Measurements with a New Photometer . J. Hartmann
The Variable Velocities of β Capricorni and ν Sagittarii in the Line of Sight W. W. Campbell
Suggestions for the Determination of Stellar Parallax by Means of Photography Frank Schlesinger
The Distribution of Stars in the Cluster Messier 13, in Hercules . . . H. K. Palmer
The Periods of the Variable Stars in the Cluster Messier 5 . . . . S. I. Bailey
Note on the Relation Between the Visual and Photographic Light Curves of Variable Stars of Short Period S. I. Bailey
The Annular Nebula *H IV.* 13 in Cygnus James E. Keeler
On the Effect of Pressure Upon the Wave-Lengths of the Lines of the Hydragen Spectrum . . . . J. Wilsing
On the Presence of Oxygen in the Atmospheres of Certain Fixed Stars David Gill
Minor Contributions and Notes

DECEMBER 1899.

Robert Wilhelm Bunsen Henry Crew
The Wave-Length of the Corona Line C. A. Young
Density of Close Double Stars Alexander Roberts
The Densities of the Variable Stars of the Algol-Type . . . Henry Norris Russell
Note on the Spectrum of P Cygni A. Bélopolsky
Apparatus and Method for the Photographic Measurement of the Brightness of Surfaces J. Hartman
The Great Sun-Spot of September 1898 J. Fényi
A Spectroscope of Fixed Deviation Ph. Pellin and André Broca
Researches on the Arc-Spectra of the Metals B. Hasselberg
Reviews

JANUARY 1900.

The Spiral Nebula *H. I, 55 PEGASI* James E. Keeler
On a New System for Spectral Photometric Work . . . . D. B. Brace
Calibration of the Slit in Spectral Photometric Measurements . . E. V. Capps
On the Escape of Gases from Planetary Atmospheres According to the Kinetic Theory . . . . S. R. Cook
On a Balancing Rheostat for Bolometers P. G. Nutting
Solar Eclipse Problems George E. Hale
Researches on the Arc-Spectra of the Metals B. Hasselberg
On the Deviations from the Law of Reciprocity for Bromide of Silver Gelatine K. Schwarzschild
On the Effect of Intermittent Exposure on Bromide of Silver Gelatine K. Schwarzschild
Minor Contributions and Notes

CHICAGO
The University of Chicago Press
LONDON: Wm Wesley & Son

Published every month except
July and September

Subscription Price
Domestic, $4.00; Foreign, $4.50

PORTRAITS

To satisfy a demand which has arisen, the publishers have arranged to issue a limited number of copies of the portraits of scientific men, that have appeared from time to time as frontispieces in the Physical Review.

The portraits are printed in photogravure, on plate paper, suitable for framing, and are for sale at the very low price of 25 cents each.

NOW READY.

JOHN TYNDALL.
HEINRICH HERTZ.
HERMAN VON HELMHOLTZ (1876).
AUGUST KUNDT.
ALFRED M. MAYER.
WILLIAM A. ROGERS.
GUSTAV WIEDEMANN
ROBERT BUNSEN

THE PHYSICAL REVIEW

A Journal of Experimental and Theoretical Physics

CONDUCTED BY

EDWARD L. NICHOLS, ERNEST MERRITT and FREDERICK BEDELL

LI. February, 1900

TABLE OF CONTENTS

The Influence of Electrification on the Surface Tension of Water and Mercury. ERNEST MERRITT and SAMUEL J. BARNETT.

An Objective Representation of the Hysteresis of Iron and Steel. KNUT ÅNGSTRÖM.

A Determination of the Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether. II. REGINALD A. FESSENDEN.

Thermodynamic Notes (No. 10); Revision of Some Thermal Data Concerning Benzene. K. TSURUTA.

An Efficient Boyle's-Law Apparatus. W. J. HUMPHREYS.

New Books: *Scheiner:* Strahlung und Temperatur der Sonne; *Gerland* und *Traumüller:* Geschichte der Physikalischen Experimentierkunst.

LII. March, 1900

TABLE OF CONTENTS

On the Metabolism of Matter in the Living Body. EDWARD B. ROSA.

A Dissociation Theory of the Electric Arc. C. D. CHILD.

The Effect of Magnetization on the Elasticity of Rods. II. JAMES S. STEVENS.

The Influence of Temperature, Pressure, Used Solutions, and Size of Anodes on the Deposit of Silver Voltameters. J. F. MERRILL.

A Lecture Demonstration in Wave Motion. WILL. C. BAKER.

An Apparatus for the Determination of the Coefficient of Expansion of Air at Constant Pressure. C. F. ADAMS.

On the Change in Length of Soft Iron in an Alternating Magnetic Field. LOUIS W. AUSTIN.

New Books: *Wentworth and Hill:* Text-book of Physics; *Franklin and Williamson:* Alternating Currents; *Aimé:* De l'influence de la pression sur les actions Chimiques; *Duhem:* Mechanique Chimique; *Miller:* American Telephone Practice.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

VOLUME X NUMBER 5

THE PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS

ERNEST MERRITT, and FREDERICK BEDELL

LIV

MAY--JUNE 1900

TABLE OF CONTENTS

The Transmission of the Ionized Exhalations of Phosphorous Through Air and Other Media C. BARUS 257
An Hermetrically Sealed Type of Clark Standard Cell H. T. BARNES 268
Concerning Thermal Conductivity in Iron EDWIN H. HALL 277
Spark-Length of an Electric Influence Machine as Modified by a Small Spark from the Negative Side W. J. HUMPHREYS 311
Index 318

COPYRIGHT, 1899, BY THE MACMILLAN COMPANY

PUBLISHED FOR CORNELL UNIVERSITY
BY THE MACMILLAN COMPANY
LANCASTER, PA., AND NEW YORK
LONDON: MACMILLAN AND CO., LTD
BERLIN: MAYER AND MUELLER

THE PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS
ERNEST MERRITT, AND FREDERICK BEDELL

Two volumes of THE PHYSICAL REVIEW are published annually, these volumes beginning in July and January, respectively, and containing at least five numbers each. The price of subscription is two dollars and fifty cents a volume (five dollars a year), or fifty cents a number. Subscriptions should be sent to the publishers. THE MACMILLAN COMPANY, 41 *North Queen Street*, *Lancaster*, *Pa.*, or 66 *Fifth Avenue*, *New York;* Messrs. MACMILLAN & CO., LTD., *London;* or to Messrs. MAYER AND MUELLER, *Berlin*.

Previous to Volume V. (July–December, 1897) THE PHYSICAL REVIEW was published in annual volumes, each containing six bimonthly numbers, beginning with the July–August number, 1893. These may be obtained from the publishers at the former subscription price, three dollars per volume.

Subscribers who desire to have their copies of the REVIEW sent to them with the edges cut, can so obtain them by informing the publishers to that effect.

Correspondence relating to contributions should be addressed to the editors, at *Ithaca*, *New York*.

Manuscript intended for publication in THE PHYSICAL REVIEW must be communicated by the author; when publication in other journals is contemplated, notice to that effect should be given.

The authors of original articles published in the REVIEW will receive one hundred separate copies in covers, for which no charge will be made; additional copies, when ordered in advance may be obtained at cost.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

TEXT-BOOKS

Polyphase Electric Currents and Alternate Current Motors

By Prof. S. P. THOMPSON. New Edition. Colored Plates - **$5.00**

Steam Engines and Boilers

An Elementary Text.Book on. By Prof. J. H. KINEALY, D.E. - **$2.00**

Railway Curves and Earthwork

By Prof. C. FRANK ALLEN. Leather - - - - - - **$2.00**

The Steam Engine as a Thermodynamic Machine

By Prof. J. COTTERILL - - - - . - - - - - - **$4.50**

The Engineers' Sketchbook of Mechanical Movements, Devices, Appliances, etc.

By T. W. BARBER, M.E. - . - - - - - - - - **$4.00**

Hydraulics and Hydraulic Machinery

By R. G. BLAINE - - - - - - - - - - - - - **$5.00**

Practical Solid Geometry

A Text-Book for Military Students, etc. By Capt. E. H. DEV. ATKINSON, R.E,

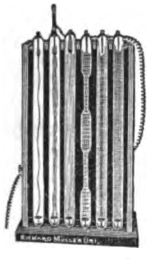
Send for our New List of Text-books.

SPON & CHAMBERLAIN, PUBLISHERS 12 Cortlandt St., NEW YORK

RICHARD MÜLLER-URI Braunschweig (Germany)

Manufacturer of

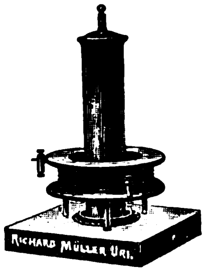
Blown Glass for Scientific Purposes


RICHARD MÜLLER URI.

Cross' Vacuum Scale

All Kinds of Lecture Apparatus and Supplies for Chemical and Physical Laboratories

Chas. R. Cross' Vacua Scale—Röntgen Tubes—McFarlan Moore Apparatus—Elster & Geitel's Transformer without Oil Insulation and Tesla Series—Spectrum Tubes (Argon, Helium) with pure Gases or Solids—Electrical Tubes after Arons, Braun, Crookes, Ebert, Geissler, Goldstein, Hittorf, Lecher, Lenard, Puluj, Zehender—Radiometers—Photo-Electric Cells and Apparatus (Selenium, etc.), Electrometer Exner.—Atmospheric Electricity Apparatus.

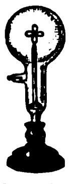

RICHARD MÜLLER URI.

Tesla Transformer (*Elster & Geitel*)

Apparatus for Gas-Analysis and Titration

Sole Maker and Furnisher of Patented Constructions:

115874 Röntgen-Tube for low tension currents
112835 Current Demonstrating Apparatus
58795 Perforator (Extractor)
38217 Sterilizor



Tesla Cross

Best Chemical Glassware

Professor Weber's Glass and Jena Normal Glass

Catalogues and Special Prospectus on Desire

WM. GAERTNER & CO.

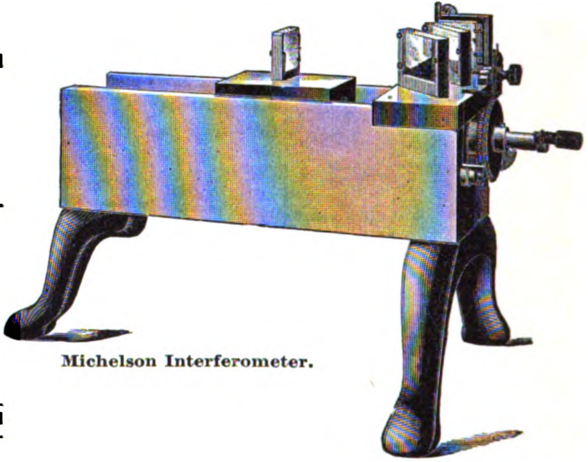
Astronomical & Physical Apparatus

5347 and 5349 Lake Avenue, CHICAGO

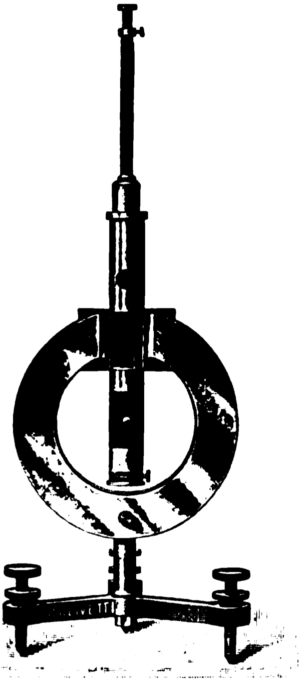
SPECIALTIES

- **Standard Apparatus of new and improved designs**
- **Astronomical Telescopes**
- **Spectroscopes**
- **Micrometers**
- **Michelson Interferometers**
- **Reading-microscopes and Telescopes**
- **Bolometers**
- **Heliostats**
- **Dividing Engines, Comparators**
- **General Laboratory Apparatus**
- **Universal Laboratory-supports**

Special attention paid to the construction of instruments for original investigation after our own or furnished designs.



Michelson Interferometer.



Professors and Assistants

in electrical laboratories don't like to give students fine galvanometers because such instruments are liable to be spoiled.

They are perfectly right

Students

don't like to work with cheaply made instruments, because they are not correct and are troublesome to handle.

They are perfectly right

Use Olivetti's Reflecting Galvanometers

and you will have high-grade up-to-date instruments which don't fear hard service.

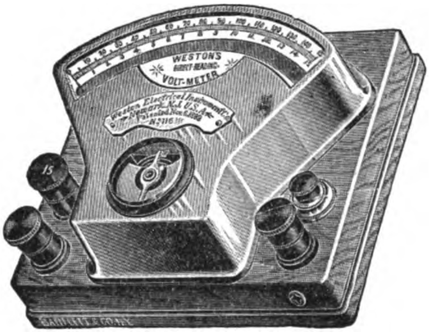
Catalogues and prices for the asking

CAMILLO OLIVETTI
Maker of Electrical Instruments
IVREA, ITALY

WESTON

STANDARD
PORTABLE
DIRECT-READING

VOLTMETERS	GROUND DETECTORS AND CIRCUIT TESTERS
MILLIVOLTMETERS	WATTMETERS
VOLTAMMETERS	OHMMETERS
AMMETERS	PORTABLE GALVANOMETERS
MILLAMMETERS	



WESTON STANDARD PORTABLE DIRECT-READING VOLTMETER FOR DIRECT CURRENT

Our Portable Instruments are recognized as The Standard the world over. The Semi-Portable Laboratory Standards are still better. Our Station Voltmeters and Ammeters are unsurpassed in point of extreme accuracy and lowest consumption of energy

---

Weston Electrical Instrument Co.

114-120 William St., NEWARK, N. J., U. S. A.

STANDARD BOOKS ON ELECTRICITY AND MAGNETISM

The Theory of Electricity and Magnetism

By A. G. WEBSTER, A.B. (Harv.), Ph.D. (Berol.)

Assistant Professor of Physics, Director of the Physical Laboratory, Clark University, Worcester, Massachusetts

8vo. Cloth. pp. xii + 576. Price, $3.50, net

Elementary Lessons in Electricity and Magnetism

By SILVANUS P. THOMPSON, D.Sc., B.A., F.R.A.S.

New Edition With Illustrations. 16mo. Cloth. $1.40, net.

"I consider Thompson the best book of its size and nature on the subject of electricity, and have had frequent occasion to recommend it to friends and students. It is the only book that is not simply a earrangement or reproduction of the old treatise."

—WILLIAM A. HALLOCK, Ph.D., *Columbia University, New York City.*

The Principles of the Transformer

By FREDERICK BEDELL, Ph.D.

Assistant Professor of Physics in Cornell University

8vo. Cloth. 250 Illustrations. Price, $3.25, net

This work constitutes a systematic treatise on the alternating Current Transformer, and a logical exposition of the principles involved, of the most modern methods of transformer design, construction, and testing, and of transformer systems of distribution.

A Text-Book on Electro-Magnetism

AND THE CONSTRUCTION OF DYNAMOS

By DUGALD C. JACKSON, B.S., C.E.

Professor of Electrical Engineering, University of Wisconsin

12mo. Cloth. Price, $2.25, net

Alternating Currents and Alternating Current Machinery

By DUGALD C. JACKSON, C.E. *University of Wisconsin* and JOHN P. JACKSON, M.E. *Pennsylvania State College*

12mo. Cloth. pp. xvii + 729. Price, $3.50, net

"I like the way in which they develop their theory and make practical applications. I feel sure at the book will receive a hearty welcome"

—GEO. W. PATTERSON, Jr., *Asst. Professor of Physics, University of Michigan*

The Alternating Current Circuit

By W. P. MAYCOCK, M.T.E.E.

An Introductory and Non-Mathematical Book for Engineers and Students. With 51 Illustrations, Index, and Ruled Pages for Notes.

16mo. Cloth. pp. viii + 102. Price, $1.00

The Storage Battery

A PRACTICAL TREATISE ON THE CONSTRUCTION, THEORY AND USE OF SECONDARY BATTERIES,

By AUGUSTUS TREADWELL, Jr.

Cloth. 12mo. pp. xx + 257. Price, $1.75, net

JUST READY

Electricity in Town and Country Houses

By PERCY E. SCRUTTON.

Second Edition. Fully Illustrated. 12mo. Cloth. pp. xii + 148. Price, $1.00

A brief and simple statement of the first principles of the application of electricity to the needs of the home, for heating, cooking, lightning, etc. A description is given of the plants required for towns of various size, and there are numerous illustrations showing examples of electric appliances. It is a popular and untechnical book, and intended for those who have not the time necessary for a study of more strictly scientific works.

THE MACMILLAN COMPANY

New York: 66 Fifth Avenue

BOSTON CHICAGO SAN FRANCISCO

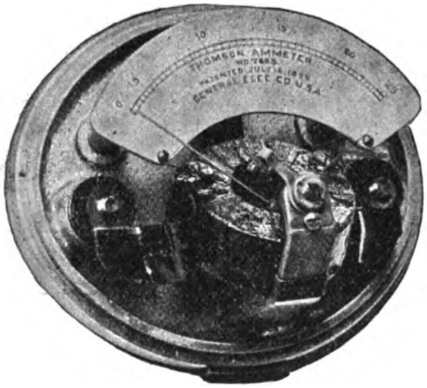
The

General Electric Company's

Pocket Ammeters

INCLINED COIL POCKET AMMETER (Covered)

of twenty-five amperes' capacity can be used upon either direct or alternating current, and gives accurate readings for experimental work in the laboratory.



INCLINED COIL POCKET AMMETER (Uncovered)

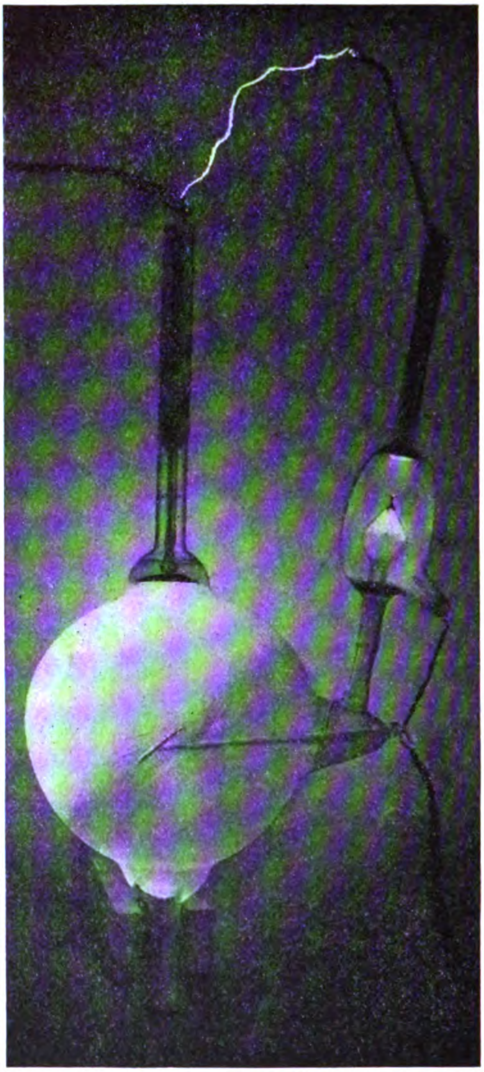
This pocket ammeter is one of a complete set of portable inclined coil electrical measuring instruments, for use in the laboratory, central station and outside work.

GENERAL ELECTRIC COMPANY

Main Office: SCHENECTADY, N. Y.

Sales Offices in All Large Cities

For CANADA: Address the CANADIAN GENERAL ELECTRIC COMPANY, Toronto, Can.



PATENT
NO.
594036
NOVEMBER
TWENTY
THIRD
1897

JOHN
SCOTT
MEDAL
FRANKLIN
INSTITUTE
1898

QUEEN
SELF-REGULATING
X-RAY TUBE

QUEEN & CO., INC., PHILADELPHIA & NEW YORK

H. LYMAN SAYEN

★ ★ "ESPECIALLY INGENIOUS" ★ ★

Prof. Dr. W.C. Röntgen.

★ ★ "MOST SATISFACTORY" ★ ★

Kelvin

Volume X. *May–June, 1900.* *Number 5.*

THE PHYSICAL REVIEW.

THE TRANSMISSION OF THE IONIZED EXHALATIONS OF PHOSPHORUS THROUGH AIR AND OTHER MEDIA.[1]

BY C. BARUS.

I. The experiments of the present paper have been made by an electrical method. They relate to the decay of the ionization of phosphoric dust in the lapse of time, and to the transmission of the same through layers of air, or other media, or barriers. I hope to decide whether a form of radiation is presumable, or whether the case is merely that of an ionized gas exhaled by slowly oxidizing phosphorus. The results which I have obtained are remarkably similar to the behavior of thorium recently investigated by Rutherford,[2] and the theory of the phenomenon might be given in accordance with the better established principles as he has done. I shall venture, however, to treat the case in a simple and direct manner, in order to present it more consistently with my last paper[3] on the same subject, in which the attempt was made to arrive at the ion velocity of phosphoric dust by a non-electrical method.

---

[1] Communicated with the permission of the Secretary of the Smithsonian Institute.

[2] Phil. Mag. (5), XLIX., p. 1, Jan., 1900.

[3] Science, XI., p. 201, Feb. 9, 1900. The ionization of phosphoric dust was known to Matteuci, and has been studied since by Neccarri and by Bidwell. I believe to have been the first to point out its remarkable activity in producing condensation, and the substance is specially interesting to me because of this property. Cf. Bull., No. 12, U. S. Weather Bureau, 1895.

Let *k* be the ion velocity of the phosphoric dust particle, normally to a charged wall, *A*. The prism of phosphoric air which reaches *A* in one second will, for any appreciable length in the direction of *k*, be at an average potential zero, and its successive layers will on the average show no charge, although saturated with the ionized agency stated. Considered non-statistically, however, the individual sections at molecular distances apart must convey immensely different charges successively, the distribution of charge or of potential on successive sections following a law something like Maxwell's, for instance, relative to the distribution of velocity among molecules in the kinetic theory of gases. To deal with the problem in this broad form would make it needlessly cumbersome, without conducing to the present purposes. Apart from this, it seems possible to obviate the question of distribution somewhat as follows:

Suppose the distribution of potential in the direction of *k* is enormously variable as compared with the potential of *A*, in such a way as to give preference neither to positive nor to negative values. *A* will lose charge if its potential is instantaneously greater than that of the section which meets it; it will receive charge in the opposite case. *A* at potential zero, therefore, neither receives nor loses charge, since the number of sections reaching *A* in an appreciable time carry equal and opposite charges in like distribution. If the charge on *A* is positive there must then be fewer layers which impart charge to *A* and more layers which withdraw it than in the preceding case, since the average charge on the layers is still zero. Hence *A* will be discharged in the lapse of time and this more rapidly as its potential is higher. Precisely the reverse will happen if *A* is negatively charged. Hence to avoid the law of distribution specified I suppose that the charge per square centimeter on *A* is relatively so small that if it is increased *n* times, there will be *n* times fewer layers to charge it and *n* times more layers

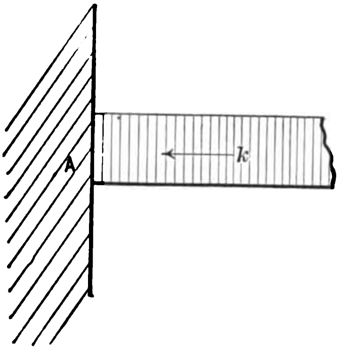


Fig. 1.

to discharge it than in the initial case. In other words I regard the charge on A per square centimeter small enough to correspond to a linear element of the law of distribution of charges along the length of the prism k. Thus the rate of A is caet. par. proportional to Q, its excess above the average charge of the advancing ionized sections which merge into A.

If n be the total number of ions per cubic centimeter, and e is proportional to the charge (positive or negative) carried by each, $k\ \ n \cdot e.$ is the total quantity of free electricity of both kinds promiscuously carried to A, per second per square centimeter. The rate of discharge of A is thus $-dQ/dt = Q \cdot n \cdot k \cdot e$, where the constant of proportionality is contained in e. The integral of the equation is $Q = Q_0 \varepsilon^{-knet}$ and the potential of the charged plate therefore

$$\varphi = \varphi_0 \varepsilon^{-knet} \tag{1}$$

in the lapse of time, t, after charging to the initial potential φ_0.

If instead of the plate A, a cylindrical surface of radius r be given like the core of a tubular condenser, for instance, the number of particles reaching the surface per square centimeter per second is $n(k + k^2/2r)e$. Thus $-dQ/dt = Q \cdot n(k + k^2/2r)e$, and

$$\varphi = \varphi_0 \varepsilon^{-n(k+k-2r)et}. \tag{2}$$

If r is infinite equation (1) is reproduced.

Corresponding equations follow for the case of charging the electrometer; but as for practical reasons my work is confined to discharging condensers, further mention is superfluous here.

The conveyance of charge into the ionized region would be similarly explained, virtually in the usual way, following Clausius. Through any interface in the ionized region two such prisms may be imagined, traveling in opposite directions. They travel to and from a boundary. The motion of the prism k is an abstraction; but if I accentuate it here, I do so because in the present investigation with phosphorus it may run closely parallel to the actual state of things. When the phosphorus grid is placed on a smooth clean surface, the position of the discs is soon marked by apparent grease spots, due to deliquescing phosphoric oxide which has diffused across. Virtually therefore an outgoing current, originating in the phosphoric source is continually kept up, whether electricity is demonstrably conveyed or not.

In the annexed table, I have given an example of the results, which is sufficient, inasmuch as the large number investigated present throughout the same character. The following figure shows the method of experiment.

B_2 is a water battery of 48 volts, permanently charging the quadrants of an Elliott electrometer, one of which is always earthed as controlled by the switch S_2. B_1 is a storage battery (20 cells suffice), one pole of which is kept earthed as determined by the switch S_1, to be closed momentarily in charging. The other terminal charges the two condensers in parallel, *MN* in the electrometer, and *CP* for the ionization experiment. The plates *M* and *P* are also permanently earthed. *N* communicates in the usual way, with the needle of the electrometer, which is thus at the same potential as the plate *C*. *P* is a phosphorus grid, consisting of two sheets of wire gauze facing each other and placed closely together so that between them thin discs of phosphorus may be secured. As the air has free access to *P* on all sides, the medium between *C* and *P* is heavily charged with phosphorus dust. The essential precautions to be observed in work of this kind will be given elsewhere. Barriers are placed for examination between *C* and *P* quite out of contact with the former plate.

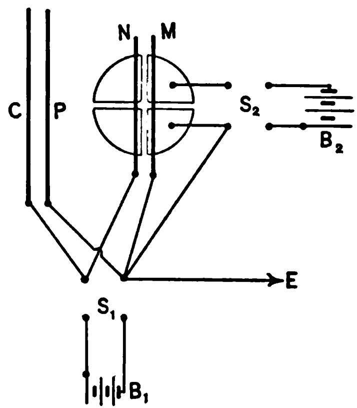


Fig. 2.

Table I. shows the leakage through the condenser *CP* when the plates are $d = 4$ centimeters apart, a relatively large distance. The exponential character of the law of discharge is manifest, but the decrease is throughout faster than can be reproduced by a simple geometric progression of the kind premised above. This might plausibly be ascribed to the waning activity of phosphorus itself in the lapse of time, and hence $c = n \cdot k \cdot e \cdot \log \epsilon$ is necessarily a decreasing quantity as n, the number of particles per cubic centim. is sup-

Table I.

Permanence of phosphorus emanation.

Absorbent Medium.	Distance apart.	Time d	Time t	Observed Potential Difference.	Computed Potential Difference.	$c = nke \cdot \log e$
Air.	4 cm.	10 h. 0 m.	0 m.	40.0 volts.		.0007
		10	10	39.0		
		15	15	39.0		
		20	20	38.6		
Air saturated with phosphoric dust.	4 cm.	10 h. 27 m.	0 m.	39 volts.	39 volts.	.0360
		28	1	35	36	
		29	2	32	33	
		30	3	29	31	
		31	4	27	28	
		32	5	25	26	
		33	6	23	24	
		34	7	21	22	
		35	8	20	20	
		36	9	19	19	
		37	10	17	17	
		38	11	16	16	
		39	12	16	15	
		40	13	15	13	
		46	19	13	—	
		51	24	13	—	
		57	30	9	—	

plied at a diminishing rate by the phosphoric source. Direct experiments negatived this supposition except for small distances, as the following examples chosen at random from many results of the same kind demonstrate.

Table II.

Decay of the ionizing activity of phosphorus.

Time	2h. 44m.	2h. 44.5m.	2h. 45m.	2h. 45.5m.	$d = 3$ cm.
Potential difference	35.6	32.4	29.4	26.8 (volts)	
Time	5h. 22m.	5h. 22.5m.	5h. 23m.	5h. 23.5m.	$d = 3$ cm.
Potential difference	35.4	32.4	29.4	27.0 (volts)	

According to the above table the rate of discharge of the freshly charged condenser before and after an interval of nearly 3 hours is not discernably different. The successive values of c were

Time	2h. 45m.	2h. 56m.	3h. 17m.	3h. 48m.	4h. 18m.	4h. 46m.	5h. 23m.
$c =$	.081	.078	.086	.078	.078	.070	.080

Fluctuation of temperature and air currents account for these irregularities. With plates 1 centim. apart the values of c were

Time	10h. 20m.	26m.	37m.	47m.	60m.	11h. 28m.	(12h. 1m.)
$c =$	.39	.39	.37	.34	.35	.34	(.33)

The last datum was obtained on readjusting the plates. Fluctuations here are greater, amounting to .05/68 or .0007 per minute, while the value .0009 is found in connection with other experiments below.

Thus it is improbable that in 15 minutes or less (usually less than 5 minutes), which outlasts the time of a single series of observations there should be any appreciable diminution of the ionizing potency of the phosphoric source. As I have not made up my mind on a satisfactory explanation I give below an exhibit of the inadequacy of the simple geometric equation. Let the observations be separated into two halves, and the constant c be computed from corresponding members, φ, t, and φ', t', of the two series. The values c and t of the table show the datum so obtained, and the mean time for which it holds. If the process is repeated to get the ratio of increments δc and δt of the values just found, the data $\delta c/\delta t$ of the table follow. These ratios are practically constant in the same series, but differ widely in different series, *i. e.*, with c. Let the mean c corresponding to each ratio be found as in the table. Then if $\delta c/\delta t$ be computed a datum is obtained, which though fully independent of c, is restricted to narrow limits. Thus in place of the above equation there follows a double exponential law

$$\varphi = \varphi_0 \varepsilon^{-Nc-\gamma t' \log \varepsilon}$$

expressing that some independent action has for the time t' been decreasing the number of particles at the rate γ. If C corresponds to c as N corresponds to n, $C/c = 10^{\gamma t' \log \varepsilon}$. The last column shows the values of C found in this way. Compared with c they are constant enough to plausibly suggest the action of some independent cause of decay, such as one would naturally attribute to the source. As this is untenable, one may note that the existence of a counter-current, or the occurrence of a species of polarization is not unreasonably suggested.

Table showing the insufficiency of a simple geometric progression. Plates 4 centim. apart.

$t = \frac{t_1 + t_2}{2}$	c	$\delta c/\delta t$	Mean c.	Mean t.	$\gamma = (\delta c/\delta t)/c$	C
4	.0374	.0023	.0327	10 h. 31 m.	.071	.0374
5	348	18	312	32	59	376
6	331	24	254	34	93	387
7	309	20	222	36	93	393
8	280					384
9	275					406
12	177					346
15	136					340
10	.0076	.0005	.0059	11 h. 51m.	.085	.0076
13	55	3	45	54	60	73
17	42					73
21	35					87

2. I next investigated the change of n, the number of particles per cubic centimeter with the distance between the condenser plates. The farther away the ions move normally from the phosphorus grid, the fewer will have survived. As at first trial I will suppose that the same relative number of ions decays per second in each layer. Thus $-dn/dx = an$ whence $n = n_0 \varepsilon^{-ax}$. If $c = n \cdot k \cdot e \cdot \log \varepsilon$ as given above, $c/c_0 = n/n_0$. The following table is a summary of a completed series of experiments computed in this way. Since for

TABLE III.

Transmission of phosphoric dust in air. Summary: $c = nke \log \varepsilon \quad c/c_0 = n/n_0$
$n = n_0 10^{-ax} \quad a = .25$.

x	Mean time.	Corrected.[1]		Corrected c observed.	Corrected c computed.	10^{-ax}
		c	c'			
1.7 cm.	11 h. 18 m.	.200	.200	.200	.200	.376
5.7	22	.023	.196	.024	.020	.038
2.5	32	.120	.187	.128	.126	.237
11.5	43	.002	.178	.002	.001	.001
1.8	12 h. 3	.130	.160	.162	.188	.355
3.7	14	.033	.150	.044	.064	.119
1.7	25	.140	.140	.200	.200	.376

---

[1] The time correction is here made linearly. At $x = 1.7$, the value of c fell from .20 to .14 in 67 minutes, *i. e.*, .0009 per minute. This result is referred to above in relation to Table II.

any two values of x, c/c' is a constant, the observed value c is to be referred to a fixed value c' ($x = 1.7$ centimeter) corrected for the apparent decreased intensity of phosphoric ionization above referred to. These values c' are given in the table as well as the values c reduced in terms of them.

The table deduces the mean value of a to be .25, whence it follows that at $1/a = 4$ centimeters, the number of ions which still survive is but 1/10 of the original number. Similar ratios $n/n_0 = 10^{-ax}$ are given in the last column. For $x = 1$ cm., 5 cm., .1 cm., the relative numbers are $n/n_0 = .56, .75, .94$ showing how rapidly saturation is reduced even close to the phosphorus grid.

The above table exhibits the general character of the phenomena fairly well. It is, however, theoretically more probable (following the suggestion originally given by Laplace in his theory of capillarity, and recently by Van der Waals in the kinetic theory of gases) to suppose the rate of decay to vary as the square of the number of ions per cubic centimeter. In other words, $-dn/dt = \beta'' n^2$ or since $c = kne \log \epsilon$, $1/c - 1/c_0 = \beta'' x/ke \log \epsilon$. Hence $c = c_0/(1 - \beta x)$, where $\beta = \beta'' c_0/ke \log \epsilon$.

I have given the constants so obtained in the following table:

TABLE IV.

Summary: $c = c_0/(1 + \beta x)$; $c/c_0 = n/n_0$; $c_0 = -.100$; $\beta = -.88$.

x	c	Corrected c observed.	Corrected c computed.
1.7	.200	.200	.200
1.8	.130	.162	.172
2.5	.120	.128	.083
3.7	.033	.044	.044
5.7	.023	.024	.025
11.5	.002	.002	.011

Hence the equation $1/c - 1/c_0 = \beta x/c_0$ where $\beta/c_0 = 8.8$ is closely in accord with the observations, but the result for small values of c is absurd, since c_0 is negative. Inasmuch as the errors in the last two tables are cumulative, better results than this were not to be expected, seeing that a reason for the apparent decay specified has not been made out. Taking the observations at their face value preference must be given to the exponential first discussed. Neither of these expressions, however, can be expected to represent the case fully, for in both differential equations all allowance for those particles which escape laterally from between the plates, is left out. The term ignored may be much more than a correction; but it would be premature to approach the question in this form before the phenomenon as a whole is better understood. Moreover, experiments with a small phosphorus grid at potential zero facing a relatively large charged plate, would offer a disposition better adapted to facilitate computation. Here the lateral loss may be made negligible.

3. The endeavor is finally to be made to decide whether the ionized particles exhaled by phosphorus are accompanied by some form of obscure radiation, or whether the reaction is restricted to an oxidation, the products of which escape at first in virtually a gaseous form. This is best done by placing barriers of thin material between the plates of the condenser, care being taken to prevent the phosphoric dust from passing around the barrier, yet allowing sufficient space for the access of air. It was my plan to fold the sheets box-like around the phosphorus grid kept at potential zero, so that escape around the edge would require a passage of 8–10 centims. to reach the other plate. This suffices, and the

TABLE V.

Transmission of phosphorus ions through barriers.

Barrier.	Thickness.	c	Date.
Tissue paper.	.006 cm.	.092	Feb. 3.
" "	"	.018	" 4, A. M.
" "	"	.007	" 4, P. M.
Writing paper.	.010 cm.	.072	Feb. 3.
" "	"	.002	" 4, A. M.
" "	"	.002	" 4, P. M.
Filter paper.	.020 cm.	.011	Feb 4, A. M.
" "	"	.008	" 4, P. M.
Aluminum foil.	.003 cm.	.003	Feb. 4, A. M.
" "	"	.002	" 4, P. M.
Insulation.		.002	
None.		.500	

plates may then be placed quite closely together on opposite sides of the barrier. Energetic action is secured at a distance apart of about 1 centim.

Porous papers, like tissue or filter paper, transmit the emanation in small quantity, depending on circumstances not easily made out. Usually not more than a few per cent. of the emanation will pass even under favorable conditions. The large values found for writing and tissue paper on the first day, were not again obtained[1] in many subsequent experiments. They may be errors, though careful inspection of the results has revealed none. It is also possible that the emanation from phosphorus may be different under qualitatively different circumstances, apart from intensity.

The general result obtained is thus definitely against any obscure radiation. The emanation passes in quasi-gaseous form with great difficulty and loss even through very thin porous barriers. It is stopped by impervious media however thin. Oiled tissue paper cuts off the ions unconditionally. Permeability seems to decrease in the lapse of time, apparently as if deliquescing phosphoric acid clogged the pores.

4. In conclusion I will briefly advert to experiments made to detect ionization in other dust producers. The method selected consisted simply in blowing dust-laden air between the plates of the condensers, the current entering at the center of the earthed plate and escaping at the edges. Thus 86,000 cubic centims. of air bubbled through concentrated sulphuric acid in about 6 minutes produced an electric leakage equivalent to but $c = .0005$; while the leakage of the condenser before the experiment was $c = .0010$. Hence the effect was increased insulation to the exclusion of all traces of ionization. The same air current tested in the color tube gave an intensely opaque field, showing its condensation producing potency to be of high order.

Ammonic polysulphide tested in like manner gave similarly negative results. About 80,000 cubic centims. passed in 5 minutes through the condenser showed no leakage whatever, whereas, the insulation leakage was originally equivalent to $c = .0006$. In this case, however, the air current produced only just perceptible darkening in the field of the color tubes, evidencing therefore but slight condensation producing tendency. The effect of this reagent on the color tube is in general very fleeting. When evaporating from the stopper of the bottle it is intensely active but only momentarily, as a rule. If however the current of air is replaced by a current of coal gas (which is itself somewhat dust-laden, as appears when the test is made with the color tube), the effect is much more persistent. Indeed the air of a large room if rendered impure in this way, reacts on the color tube for a longer interval than under any other easily producible conditions which I have found. The fleeting nuclei from ammonium sulphide seem therefore much more stable when preserved out of contact with an oxidizing medium like air, and the same is true for sulphuric acid in a measure, and for sulphur itself.

---

[1] The large values for tissue paper and larger values for filter paper ($c = .05$ to $.06$) have since been obtained under trustworthy conditions. Possibly the emanation from phosphorus may not be homogeneous in relation to temperature.

Brown University, Providence, R. I.

AN HERMETICALLY SEALED TYPE OF CLARK STANDARD CELL.

By H. T. Barnes.

OF the different types of Clark cell studied by the writer, a few hermetically sealed cells were made and briefly mentioned in another place.[1] The best of these cells, of the form known as the "sealed" cell, will be described somewhat in detail in the present paper. The advantage of a cell that can be completely closed by glass fusion is obvious. At the same time the cell should be moderately easy to construct, should embody strength and compactness, and should be perfectly portable. The present cell is primarily designed after the modified English Board of Trade model known as the B. O. T. "crystal" cell. It avoids the use of an amalgam button for negative electrode, which almost invariably ends by splitting the cell at the point where the negative terminal is fused through the glass, by the creeping of the amalgam along the wire. It includes the advantage of the "crystal" cell, *i. e.*, absence of diffusion lag, and has the extra advantage of being smaller and more sensitive. An important fact also is that the bulb of the cell containing the sensitive parts may be completely immersed in a water bath, or other form of thermostat, and at the same time is insulated thermally from the outside electrodes by the thick glass neck.

The arrangement of the cell will be best understood by reference to Fig. 1. In this the positive electrode is a platinum wire flattened at one end and amalgamated. The wire is enclosed in a glass capillary drawn from a small glass tube, shown in the figure, as the positive mercury cup. The negative electrode is a small zinc rod cast around a platinum wire and its enclosing glass capillary. This wire passes through the main fusion join *A*, and ends in the outer or negative mercury cup. As soon as the ingredients of the cell have been filled in through the open end, the final sealing of the cell is made by quickly fusing the glass in the narrow neck, *B*. By placing a small quantity of mercury in the cups, terminals, pilot leads, or other connections can be inserted for comparison or test. When the cell is to be sent away or carried any distance, the mercury can be at once removed from the cups. The contents of the cell being in a firm mass, the cell may be kept in any position.

---

[1] H. L. Callendar and H. T. Barnes, Proc. Roy. Soc., 62, 117 (1897).

To construct the outer containing vessel, a glass tube, about one centimeter in diameter, is selected and cut about 15 or 16 centimeters long. This tube is drawn in the blowpipe flame at two places; one for the point of main fusion, *A*, Fig. 1, between 4 and 5 centimeters from one end, and the other *B*, so as to leave a bulb for the ingredients about 5 centimeters long. The restriction at *A* is to be left large enough to allow the zinc rod to pass through, while that at *B* should be large enough to allow of the insertion of the ingredients, through the remaining portion of the main tube not drawn down.

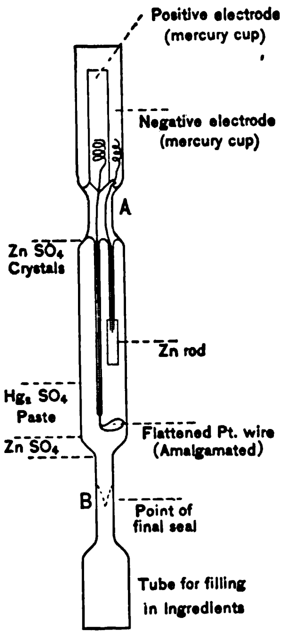

Positive electrode (mercury cup)
Negative electrode (mercury cup)
A
$ZnSO_4$ Crystals
Zn rod
Hg_2SO_4 Paste
$ZnSO_4$
Flattened Pt. wire (Amalgamated)
B
Point of final seal
Tube for filling in ingredients

Fig. 1.

The positive electrode is made by drawing down a glass tube, of the same kind of glass as the main tube, 3 or 4 millimeters in diameter, into a firm strong capillary. This is shown at *P* in Fig. 2. The capillary should be long enough to reach from the main restriction, *A*, almost to the other restriction, *B*. A platinum wire, rolled flat for about a centimeter at one end, is passed through the capillary, and sufficient length left protruding into that portion of the tube not drawn down, which serves as the positive mercury cup. The capillary tube should be melted around the wire only at the end near the flattened portion. The amalgamation of the flattened end is done by heating red hot and cooling suddenly in pure mercury.

The negative electrode is made in the following way: a small mold of glass is constructed from a glass tube about 3 millimeters in diameter and is filled with particles of absolutely pure zinc. These zinc particles are melted, and while melted, a platinum wire and a portion of its enclosing glass capillary is thrust in. On cooling and solidifying the glass mold is broken away, leaving the small zinc rod firmly attached to the capillary tube. This terminal is shown at *N*, Fig. 2. It should be long enough to reach from the seal *A* to the middle of the cell. Before inserting in the cell, the zinc rod should be scraped clean and bright and amalgamated by dipping into pure mercury upon the top of which a thin layer of dilute sulphuric acid is placed. By this means the zinc surface comes in contact with the mercury immediately after cleaning in the sulphuric acid, which is essential to ensure perfect amalgamation. After this process the rod should be most carefully washed free from every trace of acid and dried with filter paper.

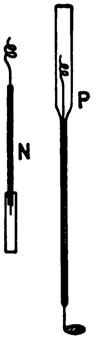


Fig. 2.

In making the main fusion, the two electrodes are placed in position by inserting them through the restriction at *A*, the positive electrode held central by means of small wedges, and the glass melted uniformly until it runs together around the two wires in a firm thick neck. The join is then carefully annealed to avoid cracking in the glass, which is liable to occur around the point where the positive mercury cup is sealed in, or at the point where the negative wire passes through. Before placing the electrodes in the cell, the ends of the wires for the mercury cups may be amalgamated.

In filling the cell, it is turned upside down and a small quantity at a time of moist zinc sulphate crystals pushed through the narrow neck by means of a small stiff glass rod. Once through the neck they can be shaken down into place. Crystals are added until the zinc rod is well covered. The paste, consisting of the usual mixture of pure washed mercurous sulphate and zinc sulphate crystals, is then pushed through in the same way. The paste should completely cover the flattened platinum wire. A few zinc sulphate crystals are then pushed through until they form a thin layer over the paste. The neck is then cleaned and dried as much as possible with filter paper, and fused together quickly in a small blowpipe flame. Very little heat is imparted to the glass beyond the neck, and what is passed to the ingredients is taken up by the thin layer of zinc sulphate crystals, thus avoiding any decomposition of the mercurous sulphate. The small bubble of air left after fusing the neck allows of the slight expansion of the materials and guards against the bursting of the cell. The completed cell, as in daily use in this laboratory, is shown by the photograph in Fig. 3, about two-thirds natural size.

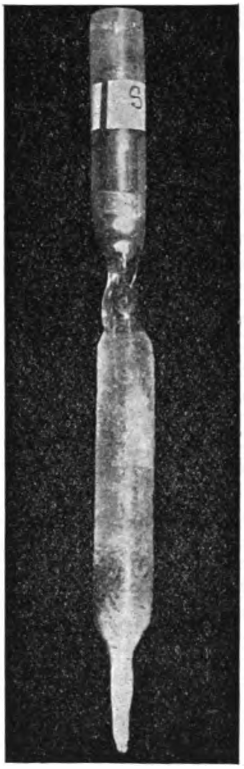


Fig. 3.

Preparation of the Ingredients.

Mercury.—For amalgamation the mercury should be, when very impure, twice distilled in vacuum. When moderately pure, a single distillation is sufficient.

Zinc Sulphate.—The ease with which the very purest anhydrouszinc sulphate is obtained from any of the standard chemical manu facturers, makes it quite unnecessary to treat the solution with zinc oxide for neutralization, as recommended by the older processes. In order to obtain the heptahydrate crystals, the anhydrous salt is dissolved in water and a solution saturated at 30° C. prepared. While at 30° a small quantity of pure washed mercurous sulphate may be added, but it is not always necessary. It would depend on whether there was a trace of zinc oxide in the anhydrous salt. It, however, will do no harm and is immediately removed by filtering the solution in a water-jacketed funnel at 30°. The clear filtrate is cooled in melting ice, and the supernatant liquid poured off from the batch of crystals produced. Should a yellow turbidity appear in the process of cooling, it shows that the solution containing the mercurous sulphate has been heated to 35° or over. It is better to avoid the yellow turbidity altogether by not heating above 30°, but if present, the crystals may be remelted and the solution filtered until completely removed. The crystals produced in the melting ice are put aside in a stock bottle.

Mercurous Sulphate.—The purest mercurous sulphate should be obtained and washed by decantation several times, with pure distilled water. After the final washing the salt is caught on a filter and drained. When moderately dry it is transferred to a mortar and an equal quantity of zinc sulphate crystals added from the stock bottle. The mixture is ground into a thick paste with some pure metallic mercury and transferred to a second stock bottle, dark-colored or otherwise protected from the action of the light by black paper.

TESTS AND COMPARSIONS.

From a careful study of the change of E.M.F. with temperature, for the various types of cells devised, the following simple formula in millivolts was found to fit the observations most closely between 0°C. and 30°C.

$$E_t = E_{15} - 1.200\,(t - 15) - 0.0062\,(t - 15)^2. \cdots \qquad (A)$$

The analogous formula finally obtained by Kahle reads

$$E_t = E_{15} - 1.19\,(t - 15) - 0.007\,(t - 15)^2,$$

which is in remarkable agreement. By formula (A) the total change between 15 and 0°C. is 16.60 millivolts, while between 15° and 30°C it is 19.40 mv. At 30° the actual observed change diverges from this simple parabolic formula, increasing more rapidly as the temperature rises to 40°, when the sudden change of hydration of the zinc sulphate causes the E.M.F. to rise and follow a different curve. The character of the temperature curve above 30° and 40° has been studied by the author, and the details of the experiments have already been published.[1] In that place it was shown that formula A, when corrected by the additional term

$$- 0.00006\,(t - 15)^3 \cdots \qquad (B)$$

holds with great accuracy over the range 15° to 40°C. Above 40° the following expression represents the hexahydrate branch of the curve

$$E_t = E_{39} - 1.000\,(t - 39) - 0.007\,(t - 39)^2 \cdots \qquad (C)$$

---

[1] H. T. Barnes, Journ. Phys. Chem., 4, 1 (1900).

It appears probable that the divergence from the simple parabolic formula (*A*) above 30° is due to some action of the mercurous sulphate at the higher points, which does not appear below 30°, and is also the cause, as has been pointed out, for the lower value given by the Clark cell for the temperature of inversion of the heptahydrate and hexahydrate (38°.78C.), as compared with the value given by solubility determinations, which places it more nearly at 40°C.

A sufficiently clear idea may be obtained of the accuracy and sensitiveness of the "sealed" cell by reference to the author's paper above referred to, on the inversion of the hepta- and hexahydrates of zinc sulphate in the Clark cell. However, various other data are available and hitherto unpublished on this form of cell, and may not be out of place here.

In Table I. are shown comparisons on several cells made at different times, with different materials. The differences in their E.M.F. are expressed in millivolts.

TABLE I.

Cell.	Jan. 1896.	Feb. 1896.	Feb. 1898.	May 1898.	Jan. 1899.
S_1	–0.11	–0.13	+0.07	+0.02	+0.00
S_3	–0.10	——	——	——	——
S_4	+0.05	+0.03	——	——	——
S_5	+0.10	+0.11	+0.13	+0.12	+0.17
S_6	+0.01	——	——	——	——
S_7	——	+0.01	——	——	——
S_{10}	——	——	–0.02	——	——
S_{11}	——	——	——	——	–0.05
nS_{11}	——	——	——	——	–0.05
S_{12}	——	——	——	+0.27	——
S_{13}	——	——	——	–0.05	——

The differences are of the order of $\frac{1}{10}$ of a millivolt; with the exception of S_{12}, which was tested rather too soon after being set up. Owing to its removal from the laboratory with S_{13} shortly after, no further tests were obtained. It is probable, however, that it may have contained a slight trace of acid from the amalgamation of the zinc, which could only be removed by very vigorous means, such as short-circuiting the cell, or otherwise by the lapse of time. In fact, one of the best methods of aging a cell and bringing it into a steady normal condition is by short-circuiting it with a copper wire for half an hour or so after setting up. The reads in Table I. show the variation from the mean of cells S_1 to S_6. Comparisons of cells S_{10} to S_{13} were made indirectly to cells S_1 to S_6 by comparison with five B.O.T. test-tube "crystal" cells that have been in the laboratory since 1895. A comparison of the means of the five sealed cells with the five crystal cells showed that the former exceeded the latter by only 0.08 millivolt. The five crystal cells have maintained their relative differences in a very satisfactory manner, as will be shown in another place.

During the progress of the author's experiments on the specific heat of water in terms of the international electrical units, two of these sealed cells were used and frequent comparisons made for a little over a year. These comparisons are given in Table II., and will serve to show the way in which these cells may be relied upon to maintain their relative differences.

TABLE II.

$S_5 - S_1$

Date of Comparison.	Difference in mv.	Temp. of Comparison.
Nov. 16, 1897.	0.10	15°
Feb. 4, 1898.	0.06	“
“ 22, “	0.02	16°
Mar. 1, “	0.03	“
“ 3, “	0.02	14°
“ 5, “	0.06	15°
“ 9, “	0.05	“
“ 10, “	0.07	“
“ 11, “	0.04	“
“ 14, “	0.07	“
“ 16, “	0.06	“
“ 23, “	0.07	“
“ 25, “	0.07	“
“ 31, “	0.09	“
Apr. 2, “	0.06	“
Apr. 12, 1898.	0.08	15°
“ 20, “	0.10	“
May 4, “	0.12	“
“ 5, “	0.10	“
“ 9, “	0.13	“
“ 14, “	0.11	“
“ 23, “	0.10	20°
“ 28, “	0.15	15°
June 10, “	0.10	21°
“ 13, “	0.19	18°
“ 17, “	0.12	“
July 23, “	0.10	20°
Sept. 10, “	0.16	19°
Oct. 14, “	0.17	14°
Dec. 14, “	0.03	15°

The variation in the difference between the two cells is so regular and consistent, never exceeding in the extreme case much more than 1 in 10,000, that over extended periods the difference can be relied upon to one or two parts in 100,000. By taking sufficient care in the temperature and electrical measurements, this is about the order of agreement of this type of cell.

In regard to the temperature change and sensitiveness, some tests made on cell S_4 are given in Table III.

TABLE III.

Time after changing temp. of cell.	Total change of temperature.	Difference in mv. observed.	Diff. in mv. calculated by formula A.
5 minutes	15 to 0°C.	+15.90	+16.60
10 "	"	+16.44	"
60 "	"	+16.62	"
10 "	15 to 30°C.	−19.37	−19.40
30 "	"	−19.50	"

On returning to 30° from 50° C., the same cell showed a difference from its value at 15° of −19.62 mv. It is probable that had the cell been left at 30° in the first test longer than half an hour it would have shown a slightly larger difference. The total change between 15° and 30° by formula (*B*) is −19.60 mv., which is much closer to the observed value.

Table IV. contains tests made on cells S_4, S_5 and S_7, between 15°.36 and 23°.83 C.

TABLE IV.

Cell.	Total change in mv. observed.	Formula A.
S_4	−10.62	−10.62
S_5	−10.69	"
S_7	−10.62	"

Cell S_5 changed slightly more than the others, but the mean of the three cells will be a little in excess of formula (*A*), as is generally the case at the higher points.

The effect of a short circuit on one of these cells was shown by connecting the terminals of cell S_4 with a short, thick wire and obtaining comparisons of its E.M.F. during recovery after the wire was removed. In two and a-half minutes after short-circuiting for seven minutes, the E.M.F. was within three-tenths of a millivolt of its original value. In ten minutes it was within two-tenths, and it had completely recovered in thirty.

For an account of the thermostats, potentiometer and temperature measuring instruments used in these tests, reference should be made to the author's earlier papers (in loc. cit.), where also the method of carrying out the experiments is described.

Macdonald Physical Laboratory, McGill University, Feb. 14, 1900.

CONCERNING THERMAL CONDUCTIVITY IN IRON.

By Edwin H. Hall.

HAVING had occasion of late to examine with some care the information given by Landoldt and Börnstein and by Winkelmann concerning the thermal conductivity, *k*, of wrought iron and the temperature coefficient, *a*, of *k*, I have discovered so many inaccuracies, especially in Winkelmann, and have been so much impressed by the inconclusiveness of the showing, that I have felt moved to write a somewhat critical résumé of the methods and results in this limited but much worked field of investigation. I am the more induced to undertake this labor by the fact that even the most recent books published in English follow the custom of quoting the results obtained by Forbes as if they were the only ones worthy of especial mention, in spite of the fact that these results, for the best known of his bars, were long since disproved and superseded by work done on the same bar in the same laboratory.

It is true that Holborn and Wien published[1] some years ago the results of a discussion somewhat similar to that which I propose, but they did not publish their discussion; and, moreover, I find that in a number of particulars, especially in regard to the value of *a*, my conclusions are different from theirs. Where so much experimental work has been done, there is room for more than one attempt to estimate the value and results of that work. It is my hope to reach conclusions which may help to direct and make profitable future investigations. The recent interesting paper[2] of Liebenow, *Zur Thermodynamik der Thermo-ketten*, may seem to bring near the possibility of determining the thermal conductivity of metals from a study of their electrical conductivity and thermoelectric properties. But, whatever may be the final outcome, one of the first effects of Liebenow's paper must be to stimulate renewed interest in the results obtained from the methods heretofore used for the measurement of thermal conductivity.

---

[1] Zeitschr. Ver. Deutsch. Ingen., Vol. 40, 1896.

[2] Wied. Annalen, June, 1899.

Landoldt and Börnstein give[1]

	Temp.	k.	Observer.
Eisen,		.1638	Neumann.
"	über 0°	.1587	Berget.
"	0°	.1988	Angström.
"	0°	.1665	Lorenz.
Schmiedeeisen,	0°	.2070	Forbes.
"	39°	.1485	H. Weber.

and the following temperature coefficients, that is, values of the constant a in the formula $k = k_0(1 + at)$:

Eisen	— 0.0002282 Lorenz.
" gewöhnl, 0 bis 300°	— 0.000611 Mitchell.
" gekühlt, 0 bis 300°	+ 0.000706 "

Winkelmann[2] gives in his table, without the dates,

Metal.	Temp.	k.	Observer.	Date.
Eisen,	0°	0.1665	Lorenz,	1881
"	0°	0.1988	Angström,	1861-4
"	über 0°	0.1587	Berget,	1890
"	ca. 15°	0.1648	Neumann,	1862
"	ca. 15°	0.1133	Wiedemann u. Franz,	1853
"	0°	0.1509	Mitchell,	1887
"	0°	0.172	Stewart,	1893
Schmiedeeisen,	0°	0.2070	Forbes,	1862-5
"	39°	0.1485	H. F. Weber,	1880

The temperature coefficients given by Winkelmann are

— 0.000038	from the work of Lorenz,
— 0.000517	" " " " Angström,
— 0.0006838	" " " " Forbes.

I shall follow in general a chronological order in my discussion, departing from it whenever there seems to be good reason for so doing.

---

[1] Physikalische Chemische Tabellen.

[2] Handbuch der Physik., part 2 of vol. 2.

WIEDEMANN AND FRANZ.

"Es wurden zwei Stangen I. und II. von 5 mm. Dicke untersucht." This is, I believe, the whole explicit description of the iron, although it can be inferred from the description of the apparatus that the length of the bars was about 50 cm.

They worked with a considerable number of metals, but obtained comparative results only. It therefore seems idle to attempt now to derive absolute values of k from their data.

They appear to have been the first to make a systematic comparison of thermal and electric conductivities, although Forbes had many years before pointed out the apparent connection between these two properties in metals. Wiedemann and Franz did not measure, even comparatively, the electric conductivity of the bars; but trusted to the values found by others for similar materials. The table given by Winkelmann, Vol. 2^2, p. 277, to show their results differs in several particulars from that given by Wiedemann and Franz, and it is so badly aligned that in some cases it is impossible to tell to which metal the numbers given apply.

It is to be observed that Landoldt and Börnstein give no absolute values on the authority of these early experimenters.

Forbes, Angström and Neumann, working each by his own peculiar method, obtained results which they published about the same time. The method of Forbes was simpler, in theory, than the others, and will be discussed first.

FORBES (and MITCHELL).

No other experiments upon thermal conductivity are so familiar to English-speaking people as those of Forbes, and the method which he originated and used is so well known that description of its general features is unnecessary here; and yet it is time for us to recognize the fact that the numerical results obtained by Forbes have only an historical interest, and that, if their publication in physical tables of the present day is to perpetuate a belief in their numerical accuracy, it would be far better to exclude them altogether from such tables. This statement I shall undertake to verify by what follows.

Forbes used two wrought-iron bars of somewhat different sizes. The better known of the two, called *D*, he describes a "a beautiful bar, 1¼ inch square and fully eight feet long." He gives its specific gravity as 7.79. The other bar was 1 inch square. Reduced to the C. G. S. system, the values which he found for *k* from the two bars were

1¼ inch bar.	1 inch bar.	
0.207	0.1536	at 0° C.
0.1567	0.1293	" 100° "

The great difference between the values of *k* given by the two bars at the same temperature has been explained as probably due to a difference in the quality of the iron; but in view of the subsequent history of bar *D*, and the general average of values obtained for iron by other experimenters, this explanation seems inadmissible.

Each bar gave a large negative temperature coefficient for *k*. Tait, after Forbes, worked with bar *D* and applied corrections, which Forbes had not done, for change of specific heat with change of temperature. The result was to make the temperature coefficient almost or quite disappear. Then Mitchell worked with the same bar. In accordance with the advice of Tait he used sometimes the ordinary method of Forbes, heating one end of the bar while all the rest of its length was freely exposed to the air of the room, and sometimes combined the heating of one end with a cooling of the middle in order to get a steeper gradient of temperature in the cooler parts of the bar. This variation of method produced a marked change in the values found for *k*.

Forbes had expressed his results in terms of the foot, minute, and thermal capacity of a cubic foot of water. Mitchell departed from this tradition far enough to express his results in terms of the foot, minute, and thermal capacity of a cubic foot of the iron at some one temperature. It was therefore not necessary for him to determine the specific heat of the iron; but he did undertake to introduce into the values obtained for *k* at different temperatures correction for change of specific heat with temperature. It seems probable that he used a somewhat too large temperature coefficient in making this correction.

Certain thermometer corrections were applied by Mitchell, the effect of which corrections was to lessen considerably the values of thermal conductivity found for high temperatures as compared with those found for low temperatures.

The numbers which follow and the words accompanying them I have copied from Mitchell's paper:

Temperature C.		0°	100°	200°	300°
Iron (Ordinary).	Not corrected for specific heat change.	.0127	.0115	.0103	.0091
	Corrected for specific heat change.	.0131	.0123	.0115	.0107
Iron (Cooled Bar.)	Not corrected for specific heat change.	.01154	.01162	.01170	.01178
	Corrected for specific heat change.	.0119	.01274	.01358	.01442

"Iron (Ordinary)" refers to the "ordinary" Forbes's method, heat applied to one end of the bar the rest of which is exposed to the air. This is the "Eisen gewöhnl" of Landoldt and Börnstein's table. "Iron (Cooled Bar)" refers to the varied method, in which the middle of the same bar was cooled by a stream of water while the end was heated. This is the "Eisen gekühlt" of the same table. The great difference between the results of these two very similar methods throws much doubt upon the accuracy of the results obtained from either or both, so far, at least, as relates to the temperature coefficient, which has a large positive value in one case and a still larger negative value in the other case.

The values given by Mitchell are to be turned into C. G. S. units by multiplying them by 15.48 $C\rho$, where C is the specific heat at some one temperature, probably the temperature of the air, and ρ is the density. Winkelmann gets the value 0.1509 at 0°, which he puts opposite the name of Mitchell, by multiplying 0.0119 ("Cooled Bar," 0°) by $C = 0.105$ and $\rho = 7.8$. This reduction is probably not very far wrong, though a somewhat higher value for C might well have been used, but in reducing the values given by

Mitchell for *k* at higher temperatures, as Winkelmann does on page 280, he multiplies each by the specific heat of iron for the higher temperature in question, 100°, 200°, etc. The values which he thus gets for these higher temperatures are therefore doubly "corrected" for change of specific heat, once by Mitchell and again by Winkelmann. Fortunately, Winkelmann does not put these values into his final table.

Landoldt and Börnstein give no value of *k* from Mitchell; but, as we have seen, they give two values of *a*, its temperature-coefficient from his work, not perceiving, we may suppose, that "Eisen gewöhnl," "Eisen gekühlt," and "Schmiedeeisen" (Forbes) are one and the same bar *D*, one of the national monuments of Scotland. The temperature-coefficients which they give are obtained correctly from the values of thermal conductivity "corrected for specific heat change," quoted above from Mitchell. Taken together these two coefficients almost completely extinguish each other; and I can not help thinking that they should be allowed to do so entirely, and disappear from tables of physical constants.

Whatever may be thought of the accuracy of the value *k* at 0°; as finally obtained by Mitchell using his "cooled bar" method, it seems plain that the values 0.207 at 0° (Forbes) and 0.1509 at 0° (Mitchell) ought not to appear in the same table without an express statement that they were found from the same bar. Indeed the history of bar *D* goes far to show that the Forbes method is not well suited for the nicer problems of thermal conductivity, the uncertainty in estimating surface emissivity being too great.

We have not sufficiently definite data for a profitable estimation of the ratio between the thermal and electric conductivity in bar *D*.

STEWART.

R. W. Stewart also followed the method of Forbes, with some variations of detail, using a bar which he describes thus: "The iron bar was a ¾-inch square bar, about 4½ feet long, of ordinary commercial wrought iron, having its surface filed up and very lightly polished with black lead to secure uniformity of radiating power." "The density of the iron was found to be 7.556 at 0°

C." The specific heat was found by Bunsen's ice-calorimeter to be represented by the formula

$$C_t = 0.1095\,(1 + .00073t),$$

t referring to the centigrade scale.

The thermometers of Forbes were replaced by a single thermo-electric couple, made of "a good soft iron wire and German silver," each wire about 0.5 mm. in diameter. The depth of the holes bored in the bar to receive the thermo-electric junction was about 1.4 cm., the diameter about 0.1 cm.

"One end of the experimental bar was immersed in a vessel of melted lead maintained at a constant temperature, and the rest of its length was exposed to the air, but protected from draughts and external radiation by a wide trough of sheet zinc." The dimensions of this trough are not given, but from the figure shown it appears to have been about 27 cm. deep, the bar being suspended so as to hang about half way between the bottom and the top, which was not covered.

"In taking a series of observations of the distribution of temperature along the bar, the heating was allowed to go on for five or six hours, and then the deflections corresponding to each hole along the bar were observed, up and down the bar, time after time, until a set of perfectly constant and concordant readings were obtained."

"The distribution of temperature along the bars [copper as well as iron was studied] in the stationary state was determined by inserting one junction successively into the small holes drilled in the bar, while the other junction was maintained at a constant temperature[1] by inserting it in a hole in a short iron bar resting on the bottom of the zinc trough. The deflections obtained were, in this way, proportional to the difference of temperature between the several points on the bar and the temperature of the surrounding air. It was found necessary to carefully insulate the ends of the wires of the couple from one another just above the junctions, and from the bars in which they were inserted; this was done by filing down the wires, inserting a thin slip of silk between them, and then whipping them around with a fine silk thread. The outer surface of the thread charred slightly at the higher temperatures, but not sufficiently to destroy its efficiency." "The rates of cooling of the bars at different temperatures were determined by heating a piece of each bar, about a foot long, in a sand-bath to a temperature higher than required, and observing its rate of cooling under the same conditions as those to which the experimental bar was exposed. For this purpose it was suspended in the position originally occupied by the long bar, the thermo-electric junction inserted in a hole drilled at its middle point, and the deflection of the galvanometer observed at regular intervals during the cooling."

---

[1] "This temperature was indicated by a thermometer placed with its bulb in a hole drilled in the [short] bar close to the small hole in which the junction was placed. In no case was the variation of temperature more than a fifth of a degree. The bar was a short massive iron bar, not the one used for cooling experiments."

Winkelmann quotes from the abstract of Stewart's work given in the *Proceedings* of the Royal Society. But in the *Transactions*, a mistake having been discovered, Stewart gives

$$k_t = 0.175\ (1 - .0015t).$$

I am of the opinion that little importance should be attached to the value of k obtained by Stewart and still less to his value of the temperature coefficient. A thermo-electric junction such as he describes, placed in a hole in the hot bar, but not in metallic contact with it, the greater part of the length of the wires being exposed to the air and the other junction being at a temperature near that of the air, would surely not have the temperature of the iron adjoining it. Moreover, we are told nothing about the location of the short bar carrying the colder junction except this, that it rested upon the bottom of the zinc trough—whether near the hot end or the cold end or the middle of the long bar, we do not know; but this one short bar was assumed to have the effective average temperature of the surroundings of the long bar and to hold the cooler junction at this same temperature.

Finally, the method of noting fall of temperature in the cooling bar, one foot long, by means of a single thermo-electric junction inserted at its middle point, is apparently inaccurate. The ends of the bar would certainly cool faster than the middle.

Near the end of his paper Stewart remarks, "Angström's method should be adopted in preference to Forbes'." Having now done with researches by the method of Forbes, I return to the consideration of work done by his contemporaries.

ANGSTRÖM.

Angström used two bars of iron; the second, from which he obtained his best known results, was 117.8 cm. long and 3.5 cm. thick. I have found no description of its quality. Angström takes 0.8862 as the product of its specific heat by its density; but he uses precisely the same quantity for his other iron bar. It does not appear that he determined either the specific heat or the density.

Angström's general method of sending waves of temperature along the bar by alternate heating and cooling of one end, the remainder of the bar being exposed to the air, and noting the fluctuations of temperature at certain chosen points, is familiar to physicists and perhaps even more familiar to mathematicians. The mathematical discussion of the method, as given by Angström, treats specific heat and thermal conductivity as constants, it being assumed that the variation of these quantities, within the range of temperature fluctuations in the part of the bar studied, is too small to affect seriously the result arrived at. As to the validity of this assumption there has been discussion, which I do not propose to renew at present.

Whatever may be the merits of Angström's method, it is easy to show that he made near the end of his second paper[1] a large numerical error, the easy correction of which will bring his value of k for iron much nearer to the values obtained by most other investigators who have attempted to determine it.

The paper just referred to in Poggendorff contains the following record of data and experimental results:

EISENSTAB.

No.	Period.	$\frac{k}{c\delta}$	Temp.	
1	32′	10.487	43°.4	Einfache Reihe: Therm. No. 2 u 3
2	32	10.429	43 .7	Doppelte Reihe: Therm. *A* und *B*
3	32	10.279	18 .9	

---

[1] Poggendorff, Vol. 118, p. 423, has a translation of the original paper.

This is followed, after a page or more devoted to the discussion of a similar record for copper, by a passage, quoted below, in which Angström professes to deduce a formula for *k*, as dependent on temperature, from the data which I have just given. A brief examination is enough to show that his formula, which has a negative temperature coefficient for *k*, cannot be derived from the numbers he refers to, which evidently give a positive temperature coefficient for *k*. Suspecting a mistake in the data as given in Poggendorff, I went to the original paper[1] and found the following table, which differs from that given above in one place of one number. We must therefore discard the data as given in Poggendorff, and consider whether the value given for *k* by Angström is consistent with the original data as here reproduced:

JERNSTÅNGEN.

No.	Period.	$\frac{k}{c\delta}$	Temp.	Anmärkningar.
1	32m	10.487	43°.4	Enkel serie; therm. no. II. o. III.
2	32	10.429	43 .7	Dubbel serie; therm. A o. B.
3	32	11.279	18 .9	" " "

The following discussion, by means of which Angström deduces his value of *k*, I quote from the article in Poggendorff, having compared the formulas in this discussion with those in the original passage and found them to be the same.

"Berechnet man den Werth von $\frac{k}{c\delta}$ aus den in der Tafel aufgestellten zwei letzen Bestimmungen für das *Eisen*, welche vollkommen vergleichbar mit einander sind, so erhält man

$$\frac{k}{c\delta} = 13.458\,(1 - 0.002874\,t)$$

und, wenn man setzt

$$c\delta = 0.8862$$

wird

$$k = 11.927(1 - 0.002874\,t)."$$

Now if we take the last two lines of the original data and calculate for them the value of $\frac{k}{c\delta}$ for 0°, we get 11.927. But this is precisely the value which Angström gets for k at 0°. According to the data referred to by Angström, he should get

---

[1] Öfversigt af Kongl. Vetenskaps-Akademiens Förhandlingar, 1862, p. 24, Stockholm.

$$k_0 = 11.927 \times 0.8862 = 10.57\,;$$

and in general

$$k = 10.57\,(1 - .002874t),$$

for this revision requires no change in the temperature-coefficient which will be found perfectly consistent with the data.

The fact appears to be that Angström, having before him the quantity 0.8862 which he was presently to use as a multiplier, accidentally let it get in as a divisor, so that its final use as a multiplier merely gave back the value of $\frac{k}{c\delta}$, instead of giving the value of k.

In at least one other particular the calculations of Angström can profitably be revised. He treats the product $c\delta$ as a constant, using the same value for one bar at 0° as for another at 50°. But I shall now take the values of $\frac{k}{c\delta}$ which he gives as the results of experiments at 43°.7 and at 18°.9, apply to these the values of $c\delta$ which appear suitable to the temperatures, and work out thus values for k at 43°.7 and at 18°.9, and finally at 0°.

There can be no great error in taking 7.8 as the value of δ at all the temperatures considered, if Angström used good wrought iron, as in all probability he did. For the values of c I shall use numbers which are obtained by a combination of those given by Lorenz with those found by Naccari.

	Lorenz.				Naccari.		
Temp.	0°	50°	75°	100°	0°	15°	100°
Sp. ht.	0.1050	0.1107	0.1136	[0.1165]	[0.1080]	0.1091	0.1151

The numbers in brackets are my extrapolations. Then

$$\begin{array}{cc} 0^\circ & 100^\circ \\ c = \left\{\begin{matrix} 0.1050 \\ 0.1080 \end{matrix}\right\} 0.1065 \qquad & \qquad c = \left\{\begin{matrix} 0.1165 \\ 0.1151 \end{matrix}\right\} 0.1158. \end{array}$$

By interpolation

$$18^\circ.9 \qquad\qquad 43^\circ.7$$
$$c = 0.1083 \qquad\qquad c = 0.1106.$$

Using these values of c, we get

$$k \text{ at } 43^\circ.7 = 10.429 \times 7.8 \times 0.1106 = 8.999,$$
$$\text{“ “ } 18^\circ.9 = 11.279 \times 7.8 \times 0.1083 = 9.528,$$

and by extrapolation k at $0^\circ = 9.93$. The general formula now becomes

$$k = 9.93(1 - .0021t),$$

in the units employed by Angström—the centimeter, gram and minute. Reducing to the ordinary system by dividing by 60, we have

$$k = 0.1655(1 - .0021t).$$

Holborn and Wien, in the paper to which reference has been made, give as the result of Angström's work

$$k = 0.1628\,(1 - 0.002874\,t),$$

but do not tell in what way they have revised his calculations.

The value 0.1988, k at 0°, which both Winkelmann and Landoldt and Börnstein give in their tables on the authority of Angström, is obtained from his 11.927 by dividing by 60. The temperature coefficient of k from Angström's work is given by Winkelmann as—0.000517. I do not understand how this value was found. A little earlier, on p. 282, Winkelmann gives Angström's result, revised for the change of $c\delta$ with temperature and reduced to C. G. S. units, as

$$k = 0.1842\,(1 - 0.001562t),$$

but here again there is nothing to show just how the numbers are derived. One value of the temperature coefficient is a little more than three times as great as the other.

Angström recognized the fact that for a satisfactory comparison of thermal and electric conductivities both quantities should be found for the same piece of metal in the same state. Accordingly, he attempted to measure the electrical resistance, x, of his bars, but encountered difficulties which baffled him, although he expressed the hope of overcoming them. Whether he finally succeeded I have not discovered. His results therefore tell us nothing directly in regard to the ratio $k \div x$.

Angström's method has not proved attractive to investigators in general. Tait tried it and gave it up. I am not aware that any-one except Angström has published results obtained by it for iron. Probably the very large value of k standing in the tables against the name of Angström has heretofore discredited his method. This consideration is now in great measure removed by the examination which has just been detailed. This objection, however, holds against all methods using temperature waves, that it is difficult for the ordinary reader, if not for every one, to see what effect will be produced in the result by small inaccuracies of theory or data. Such methods may give sufficiently accurate results, but they do not carry conviction.

NEUMANN.[1]

Neumann studied several metals. His whole published account of his work on these is contained in three pages; and the description is vague as well as brief. He used a method of variable temperature, heating one end of a bar or one section of a ring for a certain time and noting the changes of temperature after heating was stopped. Some features of his method can be seen in the work of H. Weber, which is presently to be described; for Weber avowedly followed Neumann as a guide, though perhaps with considerable deviations.

Neumann writes, "Les barres employées avaient 3 à 4 lignes de côté." The length is not given. "Dans quelques expériences j'ai aussi remplacé les barres par des anneaux." It seems not quite clear whether the results published were obtained from the bars or from the rings; but it is likely they came from the bars.

The specific gravity of the iron used was 7.74. Neumann gives 193 as the thermal conductivity of iron in the units employed, and says that this value can be reduced to the units used by Angström by multiplying by 0.0509. To reduce to C. G. S. units the

---

[1] Ann. de Chemie et Physique, Vol. 66, 1862.

193 should be multiplied by 0.0509 ÷ 60, which gives $k = 0.1637$, practically the same value which Landoldt and Börnstein put opposite the name of Neumann. I find nothing in Neumann to indicate the temperature for which his value of k is supposed to hold.

Neumann was especially interested in the ratio between thermal and electric conductivity, and he deduced comparative values of this ratio for the several metals which he used. He did not, however, determine the electric conductivity in absolute measure, or give any data from which we can now reduce it to that measure; so that the real ratio between the thermal and electric conductivities of the iron which he used cannot be found.

H. WEBER.

H. Weber "of Brunswick," not H. F. Weber as in Winkelmann, used the method of Neumann. The particular volume, No. 146, of Poggendorff, in which the original account of his work appears, is missing from the set which is at my command, and I have therefore had recourse to the translation of his paper which appeared in the *Philosophical Magazine*, vol. 44, 1872.

1. "Iron rod (annealed)," round; density 7.761; specific heat 0.1125 (temperature not given); length of bar 23.035 cm., diameter 0.75168 cm. "The density and the specific heat were specially determined, the latter according to the method proposed by Neumann."

After describing briefly the method of Angström, Weber says, "Neumann has now shown that the same problem can be treated in another way, and one more favorable for observation—and that it is more suitable for the determination of the two conducting-powers ["internal" and "external"] to subject not merely *one* end of a bar regarded as unlimited, but *both* ends of a bar of infinite length to the same periodic change of temperature in such a manner that when the two ends in the 0th period [really the *first* period of observations after the bar has been brought into a state of regular alternation] have the temperatures u_0 and u_1 they in the next following take the temperatures u_1 and u_0, in the 2d period (on the contrary) they are in the same state as in the 0th, and in the 3d as in the 1st, etc." The temperature u_1 was that of steam, u_0 about 4°.6. The length of a period was 5 minutes.

Assuming for the purposes of mathematical discussion, that the specific heat, the density, the internal and external conductivities, are constant quantities, an assumption which Weber defends only as an unfortunate necessity, he proceeds to show that determinations of k, the internal conductivity, and h, the external conductivity, can be found from observations of the temperature of the air, the temperature of the middle point of the bar (which temperature should prove to be a constant), and the difference of temperature between two other properly chosen fixed points on the bar at various intervals during each period.

Thermoelectric effects, by means of wires soldered to the bar at the points mentioned, were used to measure the temperatures required. "A thin iron wire and a thin German-silver wire were soldered in the middle of it [the iron rod] * * * opposite to each other. The ends of these two wires were soldered to two copper wires," the points of junction being near together "in the surrounding air" at a point the temperature of which was read from a sensitive thermometer. At two other points, one of which was $\frac{1}{6}$ of the length of the bar from its end, and the other $\frac{2}{6}$ of the length from the same end, "two German-silver wires were soldered to the iron bar * * * and their ends, at about 6 inches from the bar, were soldered to the two copper wires which led to the same galvanometer as the wires before mentioned." Readings of the deflections produced by this latter thermo-electric couple were taken at intervals of 15 seconds, and, as this deflection was changing rapidly during much of the time, a careful study of the characteristics of the galvanometer was necessary. The mean temperature of the air was about 5°.7; that of the middle point of the bar, 33°.1. The estimated mean temperature of the whole bar during a "period," of 5 minutes, was 39°.23, which, according to Weber, "can only be regarded as a rough approximation."

There is a tone of intelligence, frankness and thoroughness about the paper of Weber which inspires confidence. I cannot help feeling, however, that the description and discussion of the thermo-electric devices is too meagre. How thick and how long were the "thin" wires soldered to the middle of the bar? How much solder was used in making the junctions, and how great was the thickness of solder between the wires and the bar? These seem to me important questions in a case where, as here, some of the points of attachment were rapidly varying in temperature and all were much above the temperature of the room. It is unfortunate that Weber did not make such variations in his thermo-electric apparatus as to remove reasonable doubts concerning its accuracy.

On the other hand we feel somewhat reassured by the fact that he did vary considerably the surface condition of the bar, without finding any corresponding important change in the value derived for the thermal conductivity.

After giving a series of values obtained from "even periods," which agree well among themselves, and a corresponding self-accordant series from "odd periods," he says, "If we take the mean of the values of K [the internal conductivity] and H [the external conductivity] given by the even periods, and likewise of those derived from the odd periods, we obtain:

$$K = 15.14, \qquad H = 0.00266,$$

$$K = 14.56, \qquad H = 0.00266."$$

The difference between "odd" and "even" is attributed to "want of absolute coincidence of the soldering-places with the points $x = \frac{1}{6}l$ and $x = \frac{4}{6}l$." For the mean we have

$$K = 14.85 \qquad H = 0.00266.$$

"The surface of the iron bar had not the highest polish. In order to try whether the quality of the surface exerted a perceptible influence on the value of K, the previous experiments were repeated with the same iron rod after its surface had been evenly coated with soot from a gas flame. The thickness of the coating was such that the metallic surface was just perceptible through it. Here the duration of the periods amounted to 10 minutes, in order to make the observations in even and odd periods alternately, not consecutively as before." The mean results from the iron thus coated were

$$K = 14.79 \qquad H = 0.00328,$$

an extremely satisfactory agreement for K, with the previous result.

It is unfortunate that the temperature at which the specific heat was found is not given. The value used, 0.1125, seems rather high for the mean temperature of the bar. An error of two or three per cent. may occur here, making the value obtained for K too large by that amount.

Weber's values of K and H are expressed in terms of millimeters, milligrammes and seconds. To reduce to C. G. S. units we must divide K by 100 and H by 10. We are concerned with K only, from which we get

$$k = 0.1485,$$

as given by Landoldt and Börnstein and by Winkelmann. This is referred to the mean temperature of the rod, about 39°.

Weber found the ratio of the thermal conductivity to the electric conductivity, which henceforth I shall call the ratio $k \div x$, to be 2458×10^4 at 44°.3C, in the units which he used. Reduced to the C. G. S. system this ratio becomes 2458.

L. Lorenz.[1]

The iron used by Lorenz appears to have been exceptionally pure. It had a high density, 7.828 at 0°, and an electric conductivity greater than Landoldt and Börnstein quote for any other specimen of iron.

Lorenz gives from his own observations on this iron

Sp. Ht.	**Elect. Cond.**
$C_0 = 0.1050$	$x_0 = 10.374 \times 10^{-5}$
$C_{50} = 0.1107$	$x_{100} = 6.628 \times 10^{-5}$
$C_{75} = 0.1136$	

His bar was 30 cm. long and 1.5 cm. in diameter.

---

[1] Wied., N. F., 13, pp. 422 and 582, 1881.

In his main experiments Lorenz used two methods. One was that of Forbes with various improvements. The other, which he considers the more exact, consisted in measuring the time rate of increase of mean temperature throughout a certain part of a bar, with a known difference of temperature gradient at the ends of the portion considered, and with a surrounding jacket kept at a known fixed temperature by flow of cold water or steam. Moderate differences of temperature between different parts of the bar and between the bar and jacket were used. The temperature measurements were made by means of thermo-electric junctions, usually of copper and German-silver, but sometimes of copper and iron. Heat was brought into the bar by end contact with a bar of copper kept hot by means of a lamp. Of course it was necessary to know the density of the bar and its specific heat at various temperatures. Careful observations to determine these quantities were made upon the bar itself or upon a portion cut from it.

It is impossible to read the papers of Lorenz on thermal conductivity without being impressed by the thoroughness of his work and the probable accuracy of his results. But his method as a whole does not strike one as simple or easy to carry out, and there is one detail of the apparatus which suggests to me the possibility of considerable error. The thermo-electric junction commonly used consisted of a copper wire 0.01 cm. thick and a German-silver wire 0.03 cm. in diameter soldered together end to end in a straight line. This junction rested within a hole 0.04 cm. diameter bored straight through the bar from side to side, and both wires were insulated from contact with the metal of the bar, the German silver within the hole being covered with a thin wrapping of silk for this purpose. For the purpose of finding the temperature gradient leading heat into or out of the parts considered, "thermo-electric double elements were used which consisted of short German-silver wires with both ends soldered to copper wires." Now it seems probable that the ends of any one of these German-silver wires would, under the circumstances described, differ decidedly less in temperature than the parts of the bar at the two holes connected by the wire. This objection must have occurred to Lorenz, though he does not, so far as I have observed, mention it. It is easy to see that error from this defect of apparatus would in some measure eliminate itself, since it is the difference of two temperature gradients which enters into the calculations. Moreover, Lorenz states that he sometimes used copper-iron thermo-electric junctions, and sometimes junctions made by copper wires joined directly to the bar, without finding any material difference in the results obtained. Finally, it is to be said that the Forbes method, which Lorenz used for checking the results obtained by his first method, gave nearly the same values for thermal conductivity in most cases. The agreement in the case of iron was especially good, the Forbes method giving a result about 1½ per cent. smaller than the other. It is true that similar errors may affect both methods as applied by Lorenz.

The original method used by Lorenz was particularly well designed for giving the temperature coefficient of k; and the value which he found for this coefficient would be entitled to much consideration, even if it should appear that the values which he found for k were somewhat erroneous.

Lorenz found, by his preferred method, for iron

$$k_0 = 0.1665, \qquad k_{100} = 0.1627,$$

in C. G. S. units.

The temperature coefficient from these values is 0.0002282.

Landoldt and Börnstein report k and its temperature correctly from Lorenz; but Winkelmann in deducing the temperature coefficient forgets to divide by k_0.

The ratio $k \div x$, as found by Lorenz for his iron bar at 0°C., is 1605.

BERGET.[1]

Berget made first a determination of the thermal conductivity of mercury in absolute measure, and afterward found the thermal conductivity of various solid metals, including iron, by comparison with the mercury. He found the gradient of temperature in a vertical column of mercury, of which the upper end was exposed to steam and the lower end was kept cool by melting ice. A "guard ring" of mercury, exposed to the same terminal temperatures as this column, surrounded the column and prevented lateral escape of heat. The amount of heat transmitted by the shielded column was calculated from the amount of ice melted at its lower end, this latter quantity being found sometimes by use of a Bunsen ice-calorimeter, sometimes by weight. The temperature gradient was measured by means of thermo-electric couples, formed by iron wires leading through the guard cylinder into the interior column, with which they were in metallic contact.

---

[1] Compt. Rend., 1887–'90. Journal de Physique, Vol. 7, p. 2, 1888, and Vol. 9, p. 135, 1890.

The self-consistency of the results which Berget obtained for mercury is marvelous, but not greater than the admirable simplicity and directness of his method seems capable of giving. There was no need of determining specific heat or "surface conductivity."

In working with solid metals Berget superposed a column of mercury upon a column of the solid, each surrounded by its "guard ring," found the gradient of temperature in each by thermo-electric means after a stationary condition was reached, and then deduced the conductivity of the solid from that of mercury by the simplest of calculations.

The whole account of his work given by Berget is brief, and some details of apparatus or procedure are described less fully than the reader could desire; but the general method is so excellent that one can have little doubt concerning the accuracy of all parts of the investigation. It is therefore the more to be regretted that Berget has not told us the degree of purity, or even the specific gravity, of the metals which he studied. Perhaps it is not yet too late for this omission to be made good.

He determined, for solid metals, the mean thermal conductivity between 0°C. and 30°C. and declared its value to be, for iron, 0.1587.

He found the mean electrical conductivity of the same bar of iron to be, between 0°C. and 30°C., 9.41×10^{-5}.

The ratio of the mean values of the two conductivities between these limits of temperature is therefore, according to Berget,

$$k \div x = 1687.$$

This may be taken as equivalent to

$$k_{15} \div x_{15} = 1687.$$

BEGLINGER.[1]

Probably the broadest piece of work ever done by one person upon the thermal conductivity of iron is that of W. Beglinger, who determined the density, specific heat and thermal conductivity of 52 pieces of iron, including steel and cast iron, of many varieties. The following passages freely translated from his own descriptions will give an account of the general method and arrangement of apparatus.

" A broad cylinder, which in all points had the same initial temperature N, was from a determined moment, which we take as the zero point of the time, subjected to a continuous cooling by keeping its base continually at the temperature 0°, while the remainder of its surface was allowed to give off heat into a space kept likewise at the constant temperature 0°. The time-rate of fall of temperature at any point of the cylinder can be arrived at by calculation ; and after certain simplifications of the expression thus obtained for the temperature, there results the method for the determination of the internal heat conductivity." The method thus indicated is credited to H. F. Weber,[2] who had applied a somewhat similar method to the study of heat conduction in several metals[3] about 1880. " The temperature was measured thermo-electrically. Since it would not do to break the continuity of the material by means of small canals, one junction of the thermo-element was placed at the middle of the upper end surface [of the cylinder], the other junction kept at the constant temperature 0° [so-called] of the cooling water. The thermo-electric current was led through a galvanometer of such a character as to permit the variable electromotive force to be followed exactly after the lapse of a few seconds." * * * " From a given moment, after the cylinders had been heated, its lower end surface was played on by a stream of water 3 cm. thick from the public water supply (having a pressure about 7 atmospheres), while care was taken that the upper end as well as the curved surface of the cylinders should not be touched by the water."

" In the center of the upper end surface [of the cylinder] a drill hole 0.2–0.25 cm. broad and 0.15 cm. deep was filled with a piece of soft copper. In this was bored a fine hole in which the junction [of the thermo-element, of thin iron and German-silver wire] could be easily and securely placed."

---

[1] Verhandlungen des Vereins zur Beförderung des Gewerbfleisses, vol. 45, 1896, pp. 33–61.

[2] Berliner Monatsberichte, 1880, and Wied. Ann., Bd. 13.

[3] Vierteljahrsschrift der Naturforschenden Gesellschaft in Zurich, vol. 25, 1880.

The narrowest cylinders used by Beglinger had a diameter of nearly 6 cm.; the greater number had a diameter of 15.8 cm. or more. The least length was 4.44 cm.; the length of the greater number ranged from 6.5 cm. to 8 cm.

Each cylinder when in use was mounted on, or rather in, a broad thick supporting plate of iron, having at its center a hole widening conically toward the upper face and just wide enough at the bottom face to admit the cylinder. The lower end surface of the cylinder was made flush with the lower surface of the plate, being supported in that position by three small lugs attached to the under face of the plate. There could be side contact between cylinder and plate only along a strip ½ mm. high. The narrow space between the two was filled with a cement consisting of yellow wax and "Kolophonium." Above the cylinder was placed on the plate a double-walled cap of sheet copper containing water of the same temperature as the other cooling water. The water jet, about 3 cm. wide, was directed against the middle of the base of the cylinder from a distance of 3 or 4 cm. with very great force, and "erst am Rande des Gestelles [support] in seiner Bewegung in horizontaler Richtung gehemmt, nahm es seinen Weg nach unten." At the maximum this stream delivered about 3.7 liters of water per second with a velocity of 5.3 meters per second. A considerable reduction of the flow was accompanied by a perceptible loss of effectiveness of the stream.

The warming of the cylinder was effected by means of a Bunsen burner; and it lasted from 3 to 5 minutes, at times as much as 10 minutes. After the operation the under surface of the cylinder was always carefully cleaned and the water jet was put into position. Two or three minutes were thus allowed to elapse, in order that for the subsequent cooling it might be assumed that the initial temperature was everywhere the same. The warming was observed by means of the galvanometer, and "im Allgemeinen wurde nicht über 40° Temperaturdifferenz hinausgegangen." I think 40° is here a mistake for 4°; for I find later, as a typical case, "Kühlwassertemperatur 3°.3 C." * * * "$\bar{u} = 2°.0$; also gilt das Resultat für die Temperatur 5°.3 C."

As the "external" conductivity plays a certain part, though a subordinate one, in the cooling, certain direct observations upon rate of cooling by external conductivity alone were made.

In some cases the effect of removing the thermoelectric junction from the middle of the cylinder end to an excentric position was tried.

Each cylinder was tested in both of its upright positions; that is, the face which had been the base in one set of observations was made the top in another set; this gave a rough test of the homogeneity of each cylinder, which test all of the wrought-iron cylinders except one and all of the steel cylinders except one bore well. But with 14 out of 31 cast-iron cylinders the values of k obtained from the two positions differed from each other by 2% or more.

For the determination of its specific heat each cylinder was heated in a water bath to "30, 40 oder 50°," from which it was after a time quickly transferred to the calorimeter. The giving out of its heat in this calorimeter occupied from 4 to 8 minutes.

Beglinger uses the minute as his unit of time. His values of k are, therefore, to be divided by 60 in order to reduce them to the C.G.S. system, and the reduction is made in the following table, which gives the results of his work with malleable iron (Schmiede-eisen):

Schweisseisen.					
No.	l	$2R$	ρ	c	k
	cm.	cm.			
1. Schweisseisen, Gerlafingen	7.387	15.91	7.835	0.1110	0.1502
2. Sandwyk-Eisen	6.992	19.96	7.884	0.1104	0.1492
3. Siegener Schweisseisen	6.987	19.96	7.803	0.1107	0.1329
4. Schweisseisen, Gerlafingen (E.)	6.982	19.96	7.812	0.1115	0.1279
5. Frischfeuereisen, Gerlafingen	7.447	20.05	7.693	0.1112	0.1263
6. Packeteisen "	7.285	20.05	7.806	0.1110	0.1262
7. Hayange, No. 2, gewalzt	6.970	15.92	7.521	0.1134	0.1078
8. Hayange, No. 2, geschmiedet	6.987	15.91	7.550	0.1122	0.1112
Flusseisen.					
9. Flusseisen, Gerlafingen	7.275	20.05	7.849	0.1108	0.1346
10. Flusseisen	6.045	15.87	7.657	0.1125	0.1241

l = length. $2R$ = diameter.
ρ = density. c = specific heat.
k = thermal conductivity.

Beglinger does not tell, as a rule, for what temperature his values of c or of k were determined; but from various remarks it seems probable that the temperatures for k lie between 5°C. and 20°C. The mean temperatures for which c was experimentally determined probably lay ten or fifteen degrees higher. Whether the values of c, as given above, are reduced to the lower temperatures for which k holds, we are not, I believe, directly told; but it appears as if these values of c were used in finding k. The mean of the values given for c is about what Naccari and also Lorenz would give for the specific heat of iron at 50° C.

Without questioning the great value and merit of Beglinger's work, one may suspect his results for k to be affected by considerable errors. For example, the description of the method of warming the cylinder leaves it very doubtful whether its temperature was everywhere the same at the beginning of the cooling. Moreover, it is even possible to doubt whether the lower face of the cylinder, in spite of the great size and velocity of the water stream directed against it, had everywhere the temperature of the water in the stream. The parts directly attacked by the full force of the stream would, perhaps, soon attain and keep a temperature very nearly the same as that of the stream itself; but other parts would probably be less affected. We might, therefore, expect narrow cylinders to be cooled somewhat more rapidly, other things being equal, than broad cylinders. As to the possible action along the curved surface of the cylinder Beglinger remarks: "Es ist zu bemerken, dass aus der Erwärmung des Gestelles [during the warming of the cylinder by means of a Bunsen burner] kein Fehler entstehen konnte; ebensowenig ist ein Einfluss der Kittschicht, trotz vielfacher Modifikationen konstatirt worden." It is to be remarked, however, that if there is any flow out through the cement layer during the cooling, this flow would hasten the cooling more, other things being equal, in narrow cylinders than in broad cylinders. We find here, then, two possible influences tending to make k appear greater in narrow cylinders than in broad cylinders. Reasoning in this way, I was curious to see whether the results, as given by Beglinger, would give any indication of such a result. It happens that he used 11 pairs of cast-iron cylinders, the individuals of each pair being of the same material and differing, so far as was known beforehand, only in the diameter, which was about 6 cm. for one and 16 cm. for the other. Taking mean values I find

	ρ	c	k
For broad cylinders,	7.102	0.1152	0.1192
For narrow cylinders,	7.093	0.1159	0.1225

The mean k found for the narrow cylinders is, therefore, about 2.5% greater than the mean k found for the broad cylinders; and as the mean density and mean specific heat are nearly the same for one set as for the other, it seems probable that the apparent difference in the mean values of k is due to some fault in the method of experimentation.

This is not a very grave criticism of the work of Beglinger, which from its scope was necessarily somewhat rough in details; but it should be borne in mind hereafter, if there should be a disposition on the part of any one to discredit the values obtained by others on the ground that these values do not agree with any of those given by Beglinger. The maximum value of k which he found for wrought iron is 0.1502, which is a very little smaller than the value which I have found for a less dense piece of iron, about 5% less than the value found by Berget, and 10% less than the value found by Lorenz for iron of very nearly the same density, as Beglinger's iron.

Beglinger's results seem to show that, in the case of malleable iron, density is an important, though by no means the sole, factor in determining the value of k. The greatest value of k was not found in the most dense piece, No. 2, nor the least value of k in the least dense piece, No. 7; and yet, when the values of k for the ten pieces of malleable iron are plotted as ordinates with the densities as abscissas, it appears that an increase of 1% in the density is accompanied by an increase of several per cent. in the value of k, and there is an indication that the rate of increase of k rises with the increase of ρ. The other variables upon which k in soft iron depends are, according to Beglinger, very obscure. He calls attention to the fact that k in cylinder No. 1, which came directly from the iron-works, was about 15% greater than k in No. 4, a cylinder of the same material which came through a commercial firm. The density and specific heat were about the same in one of these cylinders as in the other. It seems a pity that serious effort was not made, by dissection and minute examination of these two cylinders, to discover the cause of their great difference in *k*.

It may be well to add a few words concerning the results of Beglinger for steel and cast-iron.

The 11 cylinders of the steel group were all "Flussstahlsorten." In these

ρ varied from 7.84 to 7.92,
c " " 0.1110 to 0.1135,
k " " 0.1327 to 0.0940.

There was among these no apparent connection between ρ and k.

Among the 11 cylinders four pieces are numbered twice each, once in the soft condition and once in the hard condition. "In 3 Fallen hatte der gehärtete Cylinder ein kleineres *k* als der weiche, aber nur um 11, 5 und 6% ; in eniem Falle hatte sogar der gehärtete Cylinder ein um 2% grösseres Leitungsvermögen." "Die Ergebnisse für das Härten bestätigen die Resultate des Herrn Kohlrausch nicht." In fact, Beglinger comes to the conclusion that hardness and softness in steel, whether due to its mode of manufacture or to subsequent treatment, has comparatively little influence on *k*. It is to be observed, however, that Beglinger used pieces of steel about 4.5 cm. thick, and, as he does not describe his method of hardening them, it may well be doubted whether he succeeded in making them very hard throughout.

Within the cast-iron group of 31 cylinders,

ρ varied from 6.849 to 7.217,
c " " 0.1120 to 0.1210,
k " " 0.0932 to 0.1533.

Here, as in steel, there is no apparent connection between ρ and k. It seems to be a matter of mere chance whether a very dense piece of cast iron has a high, low or medium thermal conductivity. In the eleven pairs of cast-iron cylinders already mentioned, the individuals of each pair being ostensibly of the same material, Beglinger found always a difference of k between these individuals, the smallest difference being less than ¼%, the largest 21%.

Beglinger gives the chemical analysis of none of his cylinders, and expresses a doubt as to whether such analysis would clear up the differences observed, in view of the comparatively small effect produced by the presence of a large amount of carbon. "Der gebundene Kohlenstoff drückt beim Gusseisen das k etwas hinunter im Beispiel des Cylinders No. 40 gegenüber No. 33, wo der Kohlenstoff zum grössten Theil in Form von Graphitblättchen ausgeschieden ist." "Ueber den Einfluss von Mangan, Silicium und Schwefel etc., auf das Wärmeleitungsvermögen von Stahl und Eisen fehlt jeder Anhaltspunkt."

I find from the values given by Beglinger:

for 10 pieces of soft iron the mean $k = 0.1300$,
" 11 " " steel " " " $= 0.1100$,
" 31 " " cast iron " " " $= 0.1175$.

HALL.

The general method which I have followed in measuring the thermal conductivity of a plate of wrought iron is the same as that used by Mr. Ayres and myself in our work on the thermal conductivity of cast iron.[1] Certain changes of apparatus or procedure, the most important being the substitution of platinum thermometers for thermoelectric junctions in measuring the change of temperature of the water passing over the disk, will be described in the Proceedings of the American Academy; but the character of the iron used and the results obtained will be given here.

The metal was Yorkshire wrought iron, recommended to me by an engineer friend of much experience as the softest wrought iron to be found in the Boston market. At the place of purchase the metal was described to me as containing 99.7% iron and no carbon. Chemical analysis made at my instance showed

iron	99.93%,
carbon	0.059%.

---

[1] Proceedings of American Academy of Arts and Sciences, Vol. 34, p. 283, 1899.

The values which I find for the thermal conductivity, k, of this iron are, at

28°.2C.	0.1528,
58°.3C.	0.1514,

if the specific heat of water at each of these temperatures is called 1. This would give for the temperature coefficient 0.0003, very nearly.

If the values of k are revised in accordance with the values proposed by Winkelmann[1] for the specific heat of water at the given temperatures, they become at

28°.2C.	0.1513,
58°.3C.	0.1511,

and the temperature coefficient deduced therefrom will be too small to be worth writing down, 0.0000 ?, let us say.

But the recent work of Callendar and Barnes[2] gives for the specific heat of water

0.9992	at 25°C.
0.9987	at 30°C.
0.9992	at 55°C.
1.0000	at 60°C.

If these values given by Callendar and Barnes are adopted as correct, my first values of k will stand almost without change; and I shall therefore leave them for the present without correction for variation in the specific heat of water.

My value for the temperature coefficient, 0.0003 or a little less, is, so far as it goes, a corroboration of the substantial accuracy of the temperature coefficient found by Lorenz, 0.0002282, although it may be doubted whether the last three figures of this number are of much significance. This agreement is eminently satisfactory to myself; for a comparison of the work of Lorenz with that of other investigators has convinced me that his value of the temperature coefficient is entitled to an especial degree of confidence.

---

[1] Part 2 of Vol. 2, p. 340.

[2] This REVIEW, April, 1900.

Measurements of the electrical resistance of the iron were made on 9 cylinders, each 2 cm. long and about 0.23 cm. in diameter, cut from the same great bar as the disk on which the measurements of k were made. The length of the cylinders, like the thickness of the disk, was taken parallel to the length of the bar; and the cylinders were cut from a part of the bar adjacent to that from which the disk was cut. The extreme difference in the specific resistances of these cylinders was apparently about 5 per cent. The mean specific resistance was found to be 12240 at 18° C. The mean specific conductivity, x, at the same temperature would, therefore, be 817×10^{-7} C. G. S.

Summary and Discussion.

In accordance with the preceding exposition I have made the following table of results for wrought iron, in which $\rho =$ the density, $x =$ the electric conductivity, $k =$ the thermal conductivity:

Observer.	ρ	$x \times 10^7$	k
Method of steady flow with "external conduction."			
Forbes,	7.79	?	0.207 (?) at 0°C. (see Mitchell).
Mitchell,	7.79	?	0.1509 (?) at 0°C. (sp. ht. uncertain).
Stewart,	7.556	?	0.175 (?) at 0°C.
Methods using waves or oscillations of temperature.			
Angström,	?	?	0.1655 (?) at 0°C. (sp. ht. uncertain).
Neumann,	7.74	?	0.1637(?) at ? (account too brief).
H. Weber,	7.761	604 at 44°C.	0.1485 at 39°C.
Methods of rise or fall of temperature with internal and external conduction.			
Lorenz,	7.828	1037.4 at 0°C.	0.1665 at 0°C.
Beglinger,	7.74	?	0.130 (?) at ? (method rough).
Method of steady flow with guard ring or jacket.			
Berget,	?	941 at 15°C.	0.1587 at 15°C.
Hall,	7.785	817 at 18°C.	0.1528 at 28°C.

Temperature coefficient of k.

Lorenz, $a = -0.0002282$.
Hall, $a = -0.0003$ or slightly less.

The only two values here given for a were obtained by very different methods, which, however, have this very important feature in common, that each of them used precisely the same piece of metal in finding the low temperature value of k as in finding its high temperature value, the whole apparatus being used at two different levels of temperature, with little or no relative change within it in passing from one level to the other. In the Forbes method of finding a, followed by Mitchell and Stewart, the high temperature value of k is found at one part of a bar and the low temperature value at a different part, where the quality of the metal may not be quite the same. For this reason, as well as for others which will be apparent to any one reading the preceding discussion of work by the method of Forbes, I have retained none of the values of a obtained by that method. The value to be derived from the data given by Angström seems also too uncertain for a place in this summary.

If we take -0.00025[1] as the value of a in wrought iron of high grade, I believe that we may have much confidence in the ciphers and some confidence in the first digit.

In four cases we have both k and x in C. G. S. units. Reducing to 0°C., using -0.00025 as the value of a and $x_0 = x \div (1 - 0.0051t + 0.000013t^2)$ as the temperature formula for electrical conductivity, we get in these four cases

Observer.	ρ	x	k	$k + x$
Lorenz,	7.828	10374×10^{-6}	0.1665	1605
Berget,	?	10160 "	0.1593	1568
Hall,	7.785	8958 "	0.1539	1716
H. Weber,	7.772	7540	0.1500	1989

The mean of the four values here given for k is 0.1574. The mean value deduced by Holborn and Wien was 0.156 at (?)°C.

It is plain that diminution of density is here accompanied by fall of both x and k. It will be remembered that Beglinger's results also, for soft iron, indicated a fall of several per cent. in k with a fall of one per cent. in ρ. It would be unwise, however, to draw definite numerical conclusions from the numbers before us; for all the values of x and k are in some degree uncertain, and in the case of Weber's iron even the ρ is a little dubious; for he does not tell the temperature for which ρ is 7.761, and I have assumed that it was 39°C., which he gives as the mean temperature for which he determined k. We should be in better position for speculations if we knew the value of ρ in the iron used by Berget. The x and k for this iron seem to require a high density.

---

[1] Holborn and Wien, *loc. cit.*, retaining the values of a given by Mitchell and Stewart, get, as the most probable value, $a = -0.001011$.

Although k appears to increase rapidly with increase of ρ, x increases still more rapidly, so that, with one exception, we find $k \div x$ to decrease with increase of k. This exception may be due to entirely obscure causes lying in the difference of quality of the iron used by Lorenz and Berget respectively, or it may be due to errors of measurement by one or both of these investigators. To determine this ratio within 1 % for any piece of iron would be a considerable achievement. It is worth while to inquire what values of this ratio were obtained by Lorenz and by Berget for the other metals which were examined by both of them. Brass being omitted as a too uncertain quantity, these metals were copper, tin, lead and antimony. Berget's results being given for 15°C., we must reduce them to 0°C.; and for this purpose can not do better than to take the temperature coefficients found by Lorenz. Using these we get for the ratio $k \div x$ at 0°C.

	Lorenz.	Berget.
Copper,	1574	1516
Tin,	1635	1727
Lead,	1627	1532
Antimony,	2011	1629

Thus we have, including iron, four cases out of five in which the value of $k \div x$ found by Berget was less than that found by Lorenz. I am, therefore, inclined to attribute the exception above noted, in the column $k \div x$ for iron, to errors of measurement, and to hold it as a rule that in iron the ratio $k \div x$ decreases with increase of k.

In conclusion I have to make the following suggestions:

1. That every one who undertakes to determine the thermal conductivity of any metal should determine also its electrical conductivity and thermoelectric quality, and should give in general the most definite attainable account of its physical and chemical properties.

2. That every one should have regard not merely to the probable accuracy of his method but also to its convincingness, lest his re-

sults, however accurate, may be received with doubt and so prove ineffective.

3. That the general methods used by Berget are peculiarly simple, direct and convincing; and that his probably very accurate determination of k for mercury puts at the disposal of all other investigators a standard substance by comparison with which they may, using his guard-ring method, measure the conductivity of other metals with far less labor than if they undertook independent absolute determinations.

A striking example of the uncritical use of old data relating to thermal conductivity is furnished by the paper of Liebenow referred to at the beginning of this article. Liebenow derives from his theory the following formula for the thermo-electromotive force in any metal:

$$\frac{dE}{dT} = \pm 2.04 \sqrt{\frac{RL}{T}},$$

where dE is the electromotive force in volts,

dT " difference of temperature in degrees C.,
R " specific resistance in ohms,
L " thermal conductivity,
T " absolute temperature.

He then says, " For copper one finds accordingly at 15° C., if one takes 0.8 as the specific thermal conductivity of copper,

$$\frac{dE}{dT} = \pm 2.04 \sqrt{0.8 \times 0.000001635 \div 288} = \pm 0.0001375 \text{ volts.''}$$

Why Liebenow took the value 0.8 he does not say. The values given by Winkelmann on the authority of various observers range from 0.6296 to 1.11. But having taken this value for copper he follows the curious course of calculating the thermal conductivity for iron by multiplying 0.8 by the value which Wiedemann and Franz found for the thermal conductivity of iron as compared with copper, as if there had been no improvement on the work of these observers during the past forty-five years. The electric conductivities are taken from Matthiessen's values. Accordingly Liebenow gets for iron

$$\frac{dE}{dT} = \pm 128.2 \text{ microvolts,}$$

"and one gets for the iron-copper element — 9.3 microvolts [that is, 128.2 — 137.5]; which agrees very well with observations."

To show how much importance Liebenow attaches to the case of iron and copper, I will quote one more passage. "Since the electric stream in the copper-iron element, under 276°, flows from copper to iron at the warm junction, though the electromotive force of the copper is in this case larger than that of the iron, it follows that the direction of these forces is opposite to that of the heat flow; that is, the positive electricity is in these metals, with unequal temperature, driven toward the warmer places. Moreover, since all metals investigated by Matthiessen give, in combination with copper, smaller electromotive forces than 137.5 microvolts, one must conclude that all metals when unequally heated become electrically *positive* at the warm end."

But let us see how the argument would run if we were to use the results obtained by Lorenz and by Berget, both excellent and, as compared with Wiedemann and Franz, recent experimenters.

From the data given by Lorenz we get for copper at 0° C.

$$\frac{dE}{dT} = 2.04 \sqrt{\frac{RL}{T}} = 2.04 \sqrt{\frac{k}{xT} 10^{-9}} = 0.0001549 \text{ volt,}$$

and for iron at the same temperature,

$$\frac{dE}{dT} = 2.04 \sqrt{\frac{k}{xT} 10^{-9}} = 0.0001564 \text{ volt.}$$

Thus we find $\frac{dE}{dT}$ greater for iron, the opposite of what Liebenow finds. At 15° C., the temperature used by Liebenow, the excess of $\frac{dE}{dT}$ for iron would be a little greater than at 0° C.

From Berget's data we get at 15° C. for copper

$$\frac{dE}{dT} = 2.04 \sqrt{\frac{1.045}{651300 \times 288}} = 0.0001519 \text{ volt,}$$

and for iron at the same temperature,

$$\frac{dE}{dT} = 2.04 \sqrt{\frac{0.1587}{94100 \times 288}} = 0.0001561 \text{ volt.}$$

Here again, $\frac{dE}{dT}$ is greater for iron than for copper.

However sound Liebenow's general theory may be, it seems plain that his computations should be revised.

CAMBRIDGE, May 12, 1900.

SPARK-LENGTH OF AN ELECTRIC INFLUENCE MACHINE, AS MODIFIED BY A SMALL SPARK FROM THE NEGATIVE SIDE.

By W. J. Humphreys.

ABOUT ten years ago I noticed that the discharging distance between the poles of an ordinary Toepler machine could be very greatly increased by the aid of small sparks, taken by the hand or otherwise, from the negative pole, or from any part of the machine in metallic contact with it; and also that this property was peculiar to the negative side, no such effect being produced by sparks taken from any of the positive parts.

Naturally I supposed that a description of this phenomenon, so easily and in fact often accidentally obtained from so common a machine, was to be found in the journals and larger treatises, but I have not been able to find any reference to it. I therefore venture to publish a short account of my experiments along this line, trusting that the results, if not new and interesting to all, may at least be so to many.

For the sake of convenience and clearness I refer to the accompanying sketch, in which J, J, are the two small leyden jars usually on electrical influence machines, W the wire connecting their outer coatings, A and B the discharge balls. C and D connect, one with the positive the other with the negative comb of the machine.

Let the machine be run continuously, and let the distance between A and B be gradually increased till the sparks cease to pass. If a small spark is now drawn from any part of the machine metallically connected with the *negative* pole, the dilectric between A and B will in general break down and a spark of full magnitude pass between them.

In this way the sparking distance may be increased anywhere up to seventy-five per cent. or even more, the extent of increase depending on many things as will appear from the experiments described below.

The experiments were usually made with the machine in constant action, but this was only a matter of convenience as the discharges between the poles could still be forced, by the aid of a small spark from the negative side, after the machine had come to rest, but of course before the potential had dropped very greatly.

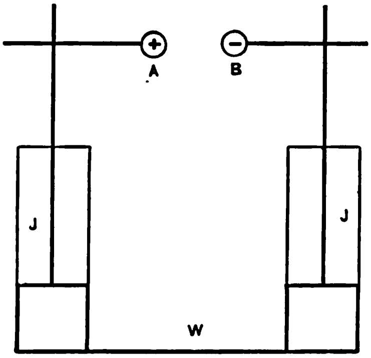


Fig. 1.

The phenomenon has been tried, and with uniform success, with several machines; among them an old type Holtz, Toepler and Wimshurst machines. Most of my observations however have been made with a two-plate Wimshurst machine, the plates being twenty inches in diameter, of ebonite and furnished with metallic sectors.

When the poles were equal in polish and otherwise it was, as one would expect, a matter of indifference which was positive and which negative, the forced spark being obtained equally well in each case.

The phenomenon has been tested under quite a number of conditions—some of which are the following. Where no statement to the contrary is made, the condensers belonging to the machine were used, and both poles kept polished.

1. Roughened negative pole. Results never good; the negative pole lost its charge so rapidly in the form of a brush that it was impossible to obtain sparks of any considerable length.

2. Polished negative pole. Results generally very good.

3. Condensers of very small capacity. Increase in spark gap slight.

4. Condensers of large capacity. Spark gap not much increased.

5. No condensers. Spark gap not affected by spark from either pole.

6. Condensers belonging to machine, and polished negative pole. Results generally excellent.

7. State of the atmosphere such as to prevent high potential, machine giving only short sparks. Results less pronounced, the poorer the working of the machine.

8. Condition of the atmosphere such as to cause unusually long sparks. Increase of spark-length ordinarily not very great.

9. Working of machine such as to give sparks of medium length, from eight to twelve centimeters with the Wimshurst machine. Increase of spark-length most pronounced, often nearly doubling the distance between the poles.

10. Poles separated very widely. In this case a spark could not be made to pass the whole distance, but each time a small spark was taken from the negative side a discharge passed from the positive pole and lost itself along many branches into the air. Nothing of this kind happened at the negative pole when a similar spark was drawn from the positive side.

11. Discharge poles removed some distance, about seventy centimeters, from the revolving disks. Results the same as when the poles were in their usual positions.

12. Outsides of condensers connected by metal rod. Results excellent.

13. Outsides of condensers connected by secondary of a large induction coil. Increase in sparking distance still very pronounced, but volume of spark diminished.

14. Outside of condensers connected by a wet string. Increase in sparking distance still decided, but volume of spark greatly diminished.

15. Condensers completely insulated from each other. Outsides temporarily grounded or not, but finally left insulated. In this case there was not the slightest indication of a spark on taking small discharges from either pole.

Various other arrangements were tried, but probably these are the most important, and from them it may be gathered that if the poles of an electrical influence machine be separated beyond their sparking distance, they will still allow a spark to pass, either as a unit from pole to pole, or as one losing itself in branches, each time a small spark is drawn from the negative side of the machine, provided that the outer coatings of the condensers are connected by a conductor; and further that nothing of the kind takes place when the condensers are insulated from each other, nor when the small spark is taken from the positive side of the machine.

It may be well to state that the size of the small spark necessary to cause the discharge between the poles is not the same under all conditions. When the poles are separated but little beyond their sparking distance it may be very slight, but has to be much larger when the distance between the poles is greatly increased.

The experiments of Lodge and others on the importance of electrical surges and oscillations in the production of sparks, would incline one to attribute the above phenomenon to some such cause. There is, however, one important objection to this explanation. If the phenomenon is really due to surges or oscillations, it is not clear why they should not be equally great, and the results just as pronounced when the side spark is taken from one pole as when it is taken from the other. Still, the fact that the phenomenon ceases when and only when the outer coatings of the jars are so insulated that there can be no oscillations or even surges between them, well nigh compells the acceptance of the above explanation, but in a distinctly modified form.

Let the jars be fully charged, and the poles separated too far to allow sparks to pass under ordinary conditions. The field of force due to these charges is a complicated one, depending of course to some extent on neighboring objects, but as a rough approximation it may be regarded as made up of lines of force between the poles *A* and *B* say of the figure, and a much greater number of lines of force from inner to outer coatings through the walls of the jars *J*, *J*. If the jars are insulated from each other a spark drawn from either *A* or *B* will decrease the strain between them. I assume the sparks to be taken with objects in connection with the earth, that is at zero potential. If, on the other hand, the outside coatings of the jars are so connected that oscillations or even one-way surges may pass between them a spark from either pole will cause an impulsive rush of the opposite charge to the other pole and a consequent rise of its potential. That the break-down takes place only when the auxiliary spark is taken from the negative side indicates that the dielectric is far more sensitive to shocks next the anode than it is next the cathode.

There are other indications, at least one of which I shall discuss in another paper, of the superior sensitiveness to electrical disturbances of the region about the positive over that about the negative pole. But this idea of sensitiveness of the dielectric next the anode, when under usual conditions, is already supported by certain experiments of Faraday, Macfarlane and De la Rue and Hugo Müller. In speaking of Faraday's experiments on discharges between spheres of different sizes, J. J. Thomson[1] says: "We may express this result by saying that when the electric field is not uniform the gas does not break down so easily when the greatest electromotive intensity is at the cathode as it does when it is at the anode."

To explain this sensitiveness at the anode, we may assume that the molecules of the dielectric between the poles are polarized and formed into Grotthus' chains, and further that these chains are more easily broken down or ionized by shocks at one end than by shocks at the other or by steady strains. This hypothesis offers a common explanation of the above phenomenon and Jaumann's discovery[2] that a gas is electrically weaker under oscillating fields than under steady ones.

In the various experiments of Jaumann and other workers on the effects of oscillations in producing sparks there are no indications, so far as I know, whether it is the positive to negative pole or the reverse impulse that produces the spark, but if the above assumptions are true it would seem to be the former and not the latter.

---

[1] Experimental Researches, p. 170.

[2] Sitzb. d. Wien Akad., 97, p. 765, 1888.

Doubtless more or less close mechanical analogies to the assumption of a Grotthus' chain more sensitive to shocks at one end than at the other could be found, but at present I shall not search for them, since the assumption is offered more as a means of coōrdinating this with other experiments, and of suggesting possible lines of further work than as a complete and final explanation.

Rouss Physical Laboratory,
University of Va., March, 1900.

INDEX TO VOLUME X.

A.

Acetylene flame, temperature of the, *Nichols*, 234.
Acoustic analyses of vowels from the phonographic record, *Bevier*, 193.
Adams, C. F., An apparatus for determining the coefficient of expansion of air at constant pressure, 178.
Aimé, G., *De l'influence de la pression sur les actions chemiques*, 190.
Air at constant pressure, apparatus for determining the coefficient of expansion of, *Adams*, 178.
Ångström, K., An objective representation of the hysteresis of iron and steel, 74.
Anodes, size of as influencing deposit of silver voltameters, *Merrill*, 167.
Arc, a dissociation theory of the electric, *Child*, 151.

B.

Baker, W. C., A lecture demonstration in wave motion, 175.
Barnes, H. T., An hermetically sealed type of Clark standard cell, 268.
  Variation of specific heat of water between 0° and 100° C.
Barnett, S. J., The influence of electrification on the surface tension of water and mercury, 65.
Barus, Carl, Scheiner's *Strahlung und Temperatur der Sonne*, 126.
  Transmission of the ionized exhalations of phosphorus through air and other media, 257.
Benzene, revision of some thermal data concerning, *Tsuruta*, 116.
Bevier, L., Jr., Acoustic analysis of vowels from the phonographic record, 193.
Book reviews.
  American telephone practice, *Miller*, 192.
  Elements of alternating currents, *Franklin and Williamson*, 189.
  Elements of physics, *Crew*, 254.
  Geschichte der physikalischen experimentierkunst, *Gerland und Traumüller*, 127.
  Influence de la pression sur les actions chemiques, *Aimé*, 190.
  Lehrbuch der experimental Physik, *Wüllner*, 63.
  Ostwald's Klassiker der exakten Wissenschaften, Nos. 99 and 100, 63.
  Strahlung und Temperatur der Sonne, *Scheiner*, 126.
  Text-book of physics, *Watson*, 255.
  Text-book of physics, *Wentworth and Hill*, 187.
  Traité élémentaire de mechanique chemique fondeé sur la Thermodynamique, *Duhem*, 191.
Boyle's law apparatus, an efficient, *Humphreys*, 123.
Boynton, Gibbs' thermodynamic model, 228.
Cajori, F., Gerland and Traumüller's Geschichte der Physikalischen Experimentierkunst, 127.
Callender, H. L., Variation of specific heat of water between 0° and 100° C., 202.
Carpenter, H. V., A new method of comparing two self-inductances, 52.
Cell, an hermetically sealed type of Clark standard, Barnes, 268.
Child, C. D., A dissociation theory of the electric arc, 151.
Copper voltameter, polarization and internal resistance of, *Moore*, 34.
Crew, H., *Elements of Physics*, 254.

D.

Density and elasticity of the ether, *Fessenden*, 1, 83.

Dissociation theory of the electric arc, *Child*, 151.
Duhem, P., *Traité élémentaire de mechanique chemique fondeé sur la Thermodynamique, Duhem*, 191.

E.

Elasticity and density of the ether, *Fessenden*, 1, 83.
Elasticity of rods, effect of magnetization on, *Stevens*, 161.
Electric and magnetic quantities, determination of nature of, *Fessenden*, 1, 83.
Electrification, influence of on the surface tension of water and mercury, *Merritt and Barnett*, 65.
Ether, elasticity and density of the, *Fessenden*, 1, 83.
Expansion of air at constant pressure, apparatus for determining the coefficient of, *Adams*, 178.

F.

Fessenden, R. A., A determination of the nature of the electric and magnetic quantities, and of the density and elasticity of the ether, 1, 83.
Franklin, W. S., Aimé's *De l'influence de la pression sur les action chemiques*, 190.
  Duhem's Traité élémentaire de Mechanique chemique fondeé sur la Thermodynamique, 191.
  Elements of Alternating Currents, 189.

G.

Gerland, E., *Geschichte der Physikalischen Experimentierkunst*, 127.

H.

Hall, E. H., Concerning thermal conductivity of iron, 277.
Hill, G. A., *Text-book of Physics*, 187.
Hodges, N. D. C., Law of distribution of velocities among gas molecules, 253.
Humphreys, W. J., An efficient Boyle's-law apparatus, 123.
  Spark-length of an electric influence machine as modified by a small spark from the negative side, 301.
Hysteresis of iron and steel, an objective representation of, *Ångström*, 74.

I.

Inductance, self, a new method of comparing, *Carpenter*, 52.
Internal resistance and polarization of the copper voltameter, *Moore*, 34.
Iron and steel, an objective representation of the hysteresis of, *Angström*, 74.
Iron, concerning thermal conductivity of, *Hall*, 276.
Iron group, magnetic molecular susceptibility of salts of the iron group, *Liebknecht and Wills*, 215.
Iron, soft, on the change in length in an alternating magnetic field, *Austin*, 180.

K.

Kimball, A. L., Crew's *Elements of Physics*, 254.

L.

Liebknecht, O., magnetic molecular susceptibility of salts of the iron group, 215.
Living body, metabolism of matter in, *Rosa*, 129.

M.

Magnetic and electric quantities, determination of nature of, *Fessenden*, 1, 83.
Magnetic field, on the change in length of soft iron in, *Austin*, 180.
Magnetic molecular susceptibility of salts of the iron group. *Liebknecht and Wills*, 215.
Magnetization, effect of, on the elasticity of rods, *Stevens*, 161.
Mercury and water, influence of electrification on the surface tension of, *Merritt and Barnett*, 65.
Merrill, J. F., The influence of temperature, pressure, used solutions, and sizes of anodes on the deposit of, 167.

Merritt, Ernest, The influence of electrification on the surface tension of water and mercury, 65.
Metabolism of matter in the living body, *Rosa*, 129.
Miller, K. B., *American Telephone Practice*, 192.
Moore, B. E., Polarization and internal resistance of the copper voltameter, 34.

N.

Nichols, E. L., Miller's *American Telephone Practice*, 192.
On the temperature of the acetylene flame, 234.
Ostwald's *Klassiker der Exakten Wissenschaften*, 63.
Watson's *Text-book of Physics*, 255.
Wüllner's *Lehrbuch der experimental Physik*, 63.

P.

Phosphorus, transmission of ionized exhalations of, through air and other media, *Barus*, 257.
Polarization and internal resistance of the copper voltameter, *Moore*, 34.
Pressure, influence of on the deposit of silver voltameters, *Merrill*, 167.

R.

Rosa, E. B., On the metabolism of matter in the living body, 129.

S.

Scheiner, *Strahlung und Temperatur de Sonne*, 126.
Self-inductances, a new method of comparing two, *Carpenter*, 52.
Silver voltameters, the influence of temperature, pressure, used solutions, and size of anodes on the deposit of, *Merrill*, 167.
Smith, H. B., Franklin and Williamson's *Elements of Alternating Currents*, 189.
Solutions, used, influence of on the deposit of silver voltameters, *Merrill*, 167.
Spark-length of an electric influence machine as modified by a small spark from the negative side, *Humphreys*, 301.
Specific heat of water between 0° and 100°C., the variation of, *Callender and Barnes*, 202.
Standard cell, Clark, on hermetically sealed type of, *Barnes*, 268.
Steel and iron, an objective representation of the hysteresis of, *Ångström*, 74.
Stevens, J. S., Effect of magnetism on the elasticity of rods, 161.
Surface tension of water and mercury, influence of electrification on, *Merritt and Barnett*, 65.

T.

Temperature, influence of, on the deposit of silver voltameters, *Merrill*, 167.
Temperature of the acetylene flame, *Nichols*, 234.
Thermal conductivity in iron, *Hall*, 277.
Thermal data concerning benzene, revision of, *Tsuruta*, 116.
Thermodynamic model, Gibbs', *Boynton*, 228.
Thompson, J. O., Wentworth and Hill's *Text-book of Physics*, 187.
Traumüller, F., *Geschichte der Physikalischen Experimentierkunst*, 127.
Tsuruta, K., Thermodynamic notes (No. 10); Revision of some thermal data concerning benzene, 116.

V.

Velocities, law of distribution of, among gas molecules, *Hodges*, 253.
Voltameter, copper, polarization and internal resistance of, *Moore*, 34.
Voltameters, silver, the influence of temperature, pressure, used solutions, and size of anodes on the deposit of, *Merrill*, 167.
Vowels, acoustic analysis of, from the phonographic record, *Bevier*, 193.

W.

Water and mercury, influence of electrification on surface tension of, *Merrill and Barnett*, 65.

Water, variation of specific heat of, between 0° and 100° C., *Callender and Barnes*, 202.

Wave motion, a lecture demonstration of, Baker, 175.

Watson, W., *Text-book of Physics*, 255.

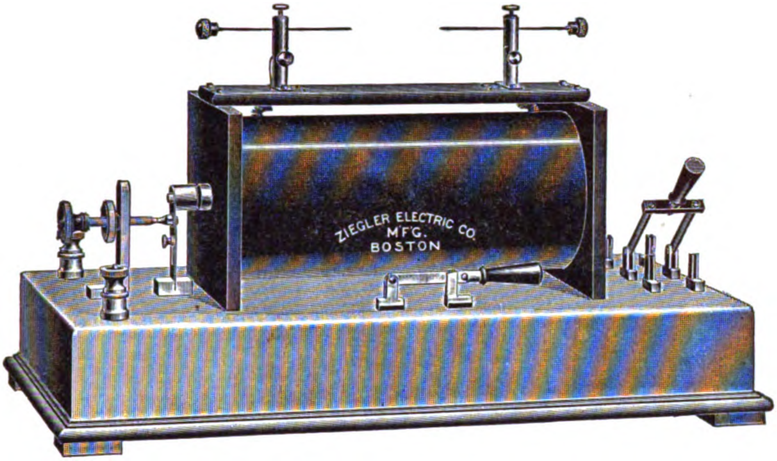
Wentworth, G. A., *Text-book of Physics*, 187.

Williamson, R. B., *Elements of Alternating Curreuts*, 189.

Wills, A. P., Magnetic molecular susceptibility of salts of the iron group, 215.

Wüllner, A., *Lehrbuch der Experimental Physik*, 63.

Ziegler Ruhmkorff Coil



Catalogue No. 1094B. Twelve-Inch Spark.

ZIEGLER ELECTRIC CO., 141 Franklin St., Boston, Mass.

Send for a Copy of Physical Catalogue No. 10.

Standard Text-Books on General Physics

Elements of Theoretical Physics

By DR. C. CHRISTIANSEN
Professor of Physics, University of Copenhagen
TRANSLATED INTO ENGLISH BY
W. F. MAGIE, Ph.D., Professor of Physics, Princeton University Cloth. 8vo. $3.25, net

Problems and Questions in Physics

CHARLES P. MATTHEWS, M.E., AND **JOHN SHEARER, B.S.**
Purdue University *Cornell University*
8vo. Cloth. pp. 247 + 4. Price $1.60, net

An Intermediate Course of Practical Physics

By A. SCHUSTER, Ph.D., F.R.S., AND **C. H. LEES, D.Sc.**
Owens College, Manchester *Owens College, Manchester*
12mo. Cloth. pp. xv + 248. Price $1.10, net

Lessons on Elementary Practical Physics

By BALFOUR STEWART, A.M., LL.D., F.R.S., AND **W. W. HALDANE GEE**
Vol. I. **General Physical Processes.** 12mo. $1.50, *net*
Vol. II. **Electricity and Magnetism.** $2.25, *net*
Vol. III. Part I. **Practical Acoustics.** $1.10, *net*
Part II. **Heat and Light.** *In Press*

Laboratory Manual of Physics and Applied Electricity

Arranged and Edited by EDWARD L. NICHOLS
Professor of Physics in Cornell University
IN TWO VOLUMES **Vol. I. Cloth. Price $3.00, net**
JUNIOR COURSE IN GENERAL PHYSICS. By ERNEST MERRITT and FREDERICK J. ROGERS. **Vol. II. Cloth. pp. 444. Price $3.25, net**
SENIOR COURSES AND OUTLINES OF ADVANCED WORK By GEORGE S. MOLER, FREDERICK BEDELL, HOMER S. HOTCHKISS, CHARLES P. MATTHEWS, and THE EDITOR.

A large proportion of the students for whom primarily this Manual is intended are preparing to become engineers, and special attention has been devoted to the needs of that class of readers. In Vol. II., specially, a considerable amount of work in applied electricity, in photometry, and in heat has been introduced.

THE MACMILLAN COMPANY New York: 66 Fifth Avenue
BOSTON CHICAGO SAN FRANCISCO

A History of Physics

In Its Elementary Branches, Including the Evolution of Physical Laboratories

By FLORIAN CAJORI, Ph.D.

Professor of Physics in Colorado College.

Cloth Cr. 8vo, $1.60 *net*.

This brief popular History gives in broad outline the development of the science of physics from antiquity to the present time. It contains also a more complete statement than is found elsewhere of the evolution of physical laboratories in Europe and America. The book, while of interest to the general reader, is primarily intended for students and teachers of physics. The conviction is growing that, by a judicious introduction of historical matter, a science can be made more attractive. Moreover, the general view of the development of the human intellect which the history of a science affords is in itself stimulating and liberalizing.

"Prof. Cajori has followed his 'History of Mathematics' with a 'History of Physics' that is *even more interesting and valuable*. . . . Every teacher and student of physics should read it and add it to his library. It shows careful study, enthusiasm and a comprehensive grasp of the subject."—*Journal of Education.*

"A most valuable contribution to the subject."—*Scientific American.*

"Probably this will prove the most successful of all Prof. Cajori's histories."
—*The Nation.*

BY THE SAME AUTHOR

A History of Mathematics

Cloth 8vo, $3.50 *net*.

"What we have a right to expect in such a handbook is an agreeable narrative of the most material events in the history of mathematics, and this Professor Cajori incontestably supplies. The book was much wanted."—*The Nation.*

"A scholarship both wide and deep is manifest in this *History of Mathematics* which the author has infused with his own ardor in this department of science."
—*Journal of Education.*

"To the student with a love for mathematical science this book will be as entertaining as a romance."
—*The Transcript*, Boston.

A History of Elementary Mathematics

Cloth 12mo, $1.50 *net*.

"A most instructive, and at the same time a very readable piece of work, full of curious facts."—*The Bookman.*

"By no means an abridged edition of *The History of Mathematics*. It is an entirely new book, giving a somewhat detailed account of the rise and progress of Arithmetic, Algebra and Geometry. The book should be read by all teachers of these subjects, and by mathematical students generally."
—*American Mathematical Monthly.*

"The product of wide and scholarly research . . . For its historical facts and its suggestiveness, this work should be read carefully by all students and teachers of mathematics."—*The Dial*, Chicago.

THE MACMILLAN COMPANY, Publishers, New York

THE CHEAPEST BOOKS IN

Science, Philosophy and Psychology

Now Publishing in America

High Grade Paper. **Large Print.** **Stitched Backs.**

THE RELIGION OF SCIENCE LIBRARY

No. 1. **The Religion of Science.** By PAUL CARUS. 25 cents (1s. 6d.).
2. **Three Introductory Lectures on the Science of Thought.** By F. MAX MÜLLER. 25 cents (1s. 6d.).
3. **Three Lectures on the Science of Language.** By F MAX MÜLLER. 25 cents (1s. 6d.).
4. **The Diseases of Personality.** By TH. RIBOT. 25 cents (1s. 6d.).
5. **The Psychology of Attention.** By TH. RIBOT. 25 cents (1s. 6d.).
6. **The Psychic Life of Micro-Organisms.** By A. BINET. 25 cts. (1s. 6d.).
7. **The Nature of the State.** By PAUL CARUS. 15 cents (9d.).
8. **On Double Consciousness.** By ALFRED BINET. 15 cents (9d.).
9. **Fundamental Problems.** By PAUL CARUS. Pp. 373. 50 cents (2s. 6d.).
10. **The Diseases of the Will.** By TH. RIBOT. 25 cents (1s. 6d.).
11. **The Origin of Language** and **The Logus Theory.** By LUDWIG NOIRÉ. 15 cents (9d.).
12. **The Free Trade Struggle in England.** By GEN. M. M. TRUMBULL. 25 cents (1s. 6d.).
13. **Wheelbarrow on the Labor Question.** 35 cents (2s.).
14. **The Gospel of Buddha.** By PAUL CARUS. 35 cents (2s.).
15. **Primer of Philosophy.** By PAUL CARUS. 25 cents (1s. 6d.).
16. **On Memory,** and **The Specific Energies of the Nervous System.** By PROF. EWALD HERING. 15 cents (9d.).
17. **The Redemption of the Brahman.** By R. GARBE. 25 cents (1s. 6d.).
18. **An Examination of Weismannism.** G. J. ROMANES. 35 cents (2s.).
19. **On Germinal Selection.** By AUGUST WEISMANN. 25 cents (1s. 6d.).
20. (Out of print.)
21. **Popular Scientific Lectures.** By ERNST MACH. 50 cents (2s. 6d.).
22. **Ancient India. Its Language and Religions,** By PROF. H. OLDENBERG. 25 cents (1s. 6d.).
23. **The Prophets of Israel.** By PROF. C. H. CORNILL. 25 cents (1s. 6d.).
24. **Homilies of Science.** By DR. PAUL CARUS. 35 cents (2s.).
25. **Thoughts on Religion.** By G. J. ROMANES. 50 cents.
26. **The Philosophy of Ancient India.** By R. GARBE. 25 cents (1s. 6d.).
27. **Martin Luther.** By GUSTAV FREYTAG. 25 cents (1s. 6d.).
28. **English Secularism.** GEORGE JACOB HOLYOAKE. 25 cents.
29. **On Orthogenesis.** By TH. EIMER. 25 cents (1s. 6d.).
30. **Chinese Philosophy.** By PAUL CARUS. 25 cents (1s. 6d.).
31. **The Lost Manuscript.** By GUSTAV FREYTAG. 60 cents (3s.).
32. **A Mechanico-Physiological Theory of Organic Evolution.** CARL VON NÄGELI. 15 cents (9d.).
33. **Chinese Fiction.** By DR. GEORGE T. CANDLIN. 15 cents (9d.).
34. **Mathematical Essays and Recreations.** By H. SCHUBERT. 25 cents (1s. 6d.).
35. **The Ethical Problem.** By PAUL CARUS. 50 cents (2s. 6d.).
36. **Buddhism and its Christian Critics.** PAUL CARUS. 50 cents (2s. 6d.).
37. **Psychology for Beginners.** HIRAM M. STANLEY. 20 cents (1s.).
38. **Discourse on Method.** RENE DESCARTES. 25 cents (1s. 6d.).
39. **The Dawn of a New Era.** PAUL CARUS. 15 cents (9d.).
40. **Kant and Spencer.** PAUL CARUS. 15 cents (9d.).

THE OPEN COURT PUBLISHING COMPANY
324 Dearborn Street, Chicago, Ill
LONDON: Kegan Paul, Trench, Trübner & Co., Ltd.

The Astrophysical Journal

AN INTERNATIONAL REVIEW OF SPECTROSCOPY AND ASTRONOMICAL PHYSICS

EDITED BY

GEORGE E. HALE
Director of the Yerkes Observatory

AND

JAMES E. KEELER
Director of the Lick Observatory

WITH THE COÖPERATION OF

THE LEADING ASTROPHYSICISTS OF THE UNITED STATES AND EUROPE

Contents of Recent Numbers

OCTOBER 1899.

The Effect of Certain Impurities on the Spectra of Some Gases . . PERCIVAL LEWIS
On Professor Keeler's Photometric Researches, by Photographic Methods, on the Nebula in Orion . . . . . . J. SCHEINER
Note on the Foregoing Article by Professor Scheiner . . . . JAMES E. KEELER
The Position of the Stars of Type IV and of the Variable Stars of Type III in Reference to the Milky Way . . . . T. E. ESPIN.
Observations of Comet Spectra W. H. WRIGHT.
The Spectroscopic Binary Capella . . . . W. W. CAMPBELL
The Variable Velocities in the Line of Sight of ε Librae, h Draconis, λ Andromedae, ε Ursae Minoris and ω Draconis W. W. CAMPBELL.
The Variable Velocity of α Ursae Minoris in the Line of Sight . . . W. W. CAMPBELL
The Variable Velocity of Polaris EDWIN B. FROST
The Wave-Length of the Green Coronal Line, and Other Data Resulting from an Attempt to Determine the Law of Rotation of the Solar Corona W. W. CAMPBELL
The Ring Nebula in Lyra JAMES E. KEELER
Pressure in the Electric Spark JOHN FRED MOHLER
Minor Contributions and Notes

NOVEMBER 1899.

On the Relative Brightness of the Planets Mars and Jupiter, from Measurements with a New Photometer . J. HARTMANN
The Variable Velocities of β Capricorni and υ Sagittarii in the Line of Sight W. W. CAMPBELL
Suggestions for the Determination of Stellar Parallax by Means of Photography FRANK SCHLESINGER
The Distribution of Stars in the Cluster Messier 13, in Hercules . H. K. PALMER
The Periods of the Variable Stars in the Cluster Messier 5 . . . . S. I. BAILEY
Note on the Relation Between the Visual and Photographic Light Curves of Variable Stars of Short Period S. I. BAILEY
The Annular Nebula *H IV.* 13 in Cygnus JAMES E. KEELER
On the Effect of Pressure Upon the Wave-Lengths of the Lines of the Hydragen Spectrum . . . . J. WILSING
On the Presence of Oxygen in the Atmospheres of Certain Fixed Stars DAVID GILL
Minor Contributions and Notes

DECEMBER 1899.

Robert Wilhelm Bunsen . . HENRY CREW
The Wave-Length of the Corona Line C. A. YOUNG
Density of Close Double Stars ALEXANDER ROBERTS
The Densities of the Variable Stars of the Algol-Type . . . . HENRY NORRIS RUSSELL
Note on the Spectrum of P Cygni A. BÉLOPOLSKY
Apparatus and Method for the Photographic Measurement of the Brightness of Surfaces J. HARTMAN
The Great Sun-Spot of September 1898 J. FÉNYI
A Spectroscope of Fixed Deviation PH. PELLIN AND ANDRÉ BROCA
Researches on the Arc-Spectra of the Metals B. HASSELBERG
Reviews

JANUARY 1900.

The Spiral Nebula *H. I, 55 PEGASI* JAMES E. KEELER
On a New System for Spectral Photometric Work . . . . D. B. BRACE
Calibration of the Slit in Spectral Photometric Measurements . . . E. V. CAPPS
On the Escape of Gases from Planetary Atmospheres According to the Kinetic Theory . . . . S. R. COOK
On a Balancing Rheostat for Bolometers P. G. NUTTING
Solar Eclipse Problems GEORGE E. HALE
Researches on the Arc-Spectra of the Metals B. HASSELBERG
On the Deviations from the Law of Reciprocity for Bromide of Silver Gelatine K. SCHWARZSCHILD
On the Effect of Intermittent Exposure on Bromide of Silver Gelatine K. SCHWARZSCHILD
Minor Contributions and Notes

CHICAGO
The University of Chicago Press
LONDON: WM. WESLEY & SON

Published every month except July and September

Subscription Price
Domestic, $4.00; Foreign, $4.50

PORTRAITS

To satisfy a demand which has arisen, the publishers have arranged to issue a limited number of copies of the portraits of scientific men, that have appeared from time to time as frontispieces in the Physical Review.

The portraits are printed in photogravure, on plate paper, suitable for framing, and are for sale at the very low price of 25 cents each.

NOW READY.

JOHN TYNDALL
HEINRICH HERTZ
HERMAN VON HELMHOLTZ (1876)
AUGUST KUNDT
ALFRED M. MAYER
WILLIAM A. ROGERS
GUSTAV WIEDEMANN
ROBERT BUNSEN

THE PHYSICAL REVIEW

A Journal of Experimental and Theoretical Physics

CONDUCTED BY

EDWARD L. NICHOLS, ERNEST MERRITT and FREDERICK BEDELL

LII. March, 1900

TABLE OF CONTENTS

On the Metabolism of Matter in the Living Body. EDWARD B. ROSA.

A Dissociation Thory of the Electric Arc. C. D. CHILD.

The Effect of Magnetization on the Elasticity of Rods. II. JAMES S. STEVENS.

The Influence of Temperature, Pressure, Used Solutions, and Size of Anodes on the Deposit of Silver Voltameters. J. F. MERRILL.

A Lecture Demonstration in Wave Motion. WILL. C. BAKER.

An Apparatus for the Detrmination of the Coefficient of Expansion of Air at Constant Pressure. C. F. ADAMS.

On the Change in Length of Soft Iron in an Alternating Magnetic Field. LOUIS W. AUSTIN.

New Books: *Wentworth and Hill:* Text-book of Physics; *Franklin and Williamson:* Alternating Currents; *Aimé:* De l'influence de la pression sur les actions Chimiques; *Duhem:* Mechanique Chimique; *Miller:* American Telephone Practice.

LIII. April, 1900

TABLE OF CONTENTS

The Acoustic Analysis of the Vowels from the Phonographic Record. LOUIS BEVIER, JR.

Note on the Variation of the Specific Heat of Water between 0° and 100° C. H. L. CALLENDAR and H. T. BARNES.

The Magnetic Molecular Susceptibility of Salts of the Iron Group. O. LIEBKNECHT and A. P. WILLS.

Gibbs' Thermodynamic Model. W. P. BOYNTON.

On the Temperature of the Acetylene Flame. EDWARD L. NICHOLS.

Note on the Law of Distribution of Velocities among Gas Molecules. N. D. C. HODGES.

New Books: *Crew:* Elements of Physics; *Watson:* Text-Book of Physics.

PUBLISHED FOR CORNELL UNIVERSITY

THE MACMILLAN COMPANY

~~REFERENCE~~